SCIENCE

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SCIENCE IN AMERICA.

THE annual meeting of the American Association for the Advancement of Science should be made the great scientific event of each year. We need special societies where students from the different centers of research may present and discuss the advances of a single science, and we need local academies where men of science working in different directions may meet on common ground. But more important than these is a meeting where all localities and all sciences are represented—a clearing house for the work of the year, where accumulations may be reported and balances adjusted. The conditions of science in America make such a meeting difficult but at the same time peculiarly desirable.

There is, indeed, no such thing as American science. We may regret that we have no school of American literature or of American art, but science is universal. It is not limited by language, nor by political and social institutions. We build us a city and a tower whose top may reach unto heaven, and our work is not stayed, though we have many languages and be scattered abroad on the face of the whole earth. But there is such a thing as science in America. We build us one city, but the stone and mortar must be taken from the ground on which we stand. We, who live and work in America, have certain advantages and certain obstacles, as compared with the great nations of Europe, with

circumstance that the discarded element may be a vowel, a consonant, or even one or more syllables, *i. e.*, a sound or combination of sounds. This method of abridgement admits of a comprehensive application of the principle of the stenographic root to which the proper affixes may be joined.

The recognition and use of the fonostenografic root introduces for the first time in shorthand systems the rational application of the natural but unconscious linguistic process of adaptation to the purposive abbreviation of words. This root (also called 'sound root' or 'phonetic radical') is not of course the etymologic root of the word or words to be written; it is defined as the 'strong and significant' element of the word; in its use the author grasps and utilizes a well known law of verbal abridgment which supplies an easy and natural rule for the simple and effective contraction and abbreviation of vocables. It would seem that in his new system the author has found the golden mean between the highest rapidity and readiest legibility—between those phonographic systems which, owing to complex structure and consequent illegibilty in practice, can be acquired only by the highly gifted or the tirelessly industrious, and those other systems which have been rendered simple in structure by 'such limitations in principles and development' as to deprive them of the requisite adaptability to the exacting needs of the reporter. In the terse language of the author, "the former class appeals to a high standard of culture; the latter addresses itself to a lower plane of mental capability and development; the former taxes too heavily the head, and the latter demands an impossible dexterity of the hand." Hitherto shorthand has commonly been pursued empirically, with little or no regard to linguistic principles, and it is a gratification to note an attempt to bring it within the domain of linguistic science.

J. N. B. HEWITT.

A Concise Handbook of British Birds. By H. KIRKE SWANN. London, John Wheldon & Co. 1896. 16°. pp. 210.

The author of this recent addition to the apparently endless series of books treating of British birds, claims for his work a unique place among its fellows on the ground of its small size and conciseness.

No space is given to remarks on the faunal position of the region under consideration, analyses of the birds which occur in it, synopses or keys to higher groups, or other preliminary or explanatory matter, it evidently being assumed that the reader is already more or less an ornithologist who will use the book as a pocket manual for ready reference.

Consequently, immediately after a 'list of genera,' we begin with 'Order Passeres,' 'Family Turdidæ,' 'Subfamily Turdinæ'—names merely—'Genus Turdus,' which is briefly defined. Then follows the species with a short statement of its 'habitat'—a term which is arbitrarily used as "meaning the region inhabited during the breeding season"—plumages, manner of occurrence, haunts, notes, nest, eggs and food, all very much condensed and with no striving for literary effect, but making useful, if not very readable, summarized biographies.

The most interesting point in Mr. Swann's book for American naturalists is his pronouncedly un-British stand in favor of trinomials. He seems to fully recognize—indeed, to glory in—the depth of his depravity, and remarks that he cannot expect to "escape censure for adopting the despised system," adding the fair challenge that, "until some of our ornithologists can suggest some other way of allowing a name to a recognised race without giving it the name of a species, I will adhere to trinomials." After this bold declaration can any one doubt that Mr. Swann's excommunication will speedily follow?

FRANK M. CHAPMAN.

SCIENTIFIC JOURNALS.

PSYCHE, AUGUST.

THE genus Orphula with its three New England species forms the subject of the continuation of A. P. Morse's paper on N. E. Tryxalinæ. A. R. Grote writes on the condition of the nomenclature of the species of Apatela. H. G. Dyar describes the early stages of *Cosmosoma auge*, and also the apparatus by which he conceives the noise made by *Dionychopus niveus* to be produced. Notice of a few recent publications completes the number.

only themselves do the best work in their power, but they will help others and will seek to make straight the way along which science must advance.

Our various scientific institutions should have the sincere support of all men of science. If our scientific journals seem less strong than those of Europe, this is not a reason for neglecting them, but rather for doing our utmost in their support. If our universities accomplish less original research than those of Germany, this should lead each to devote his best energy to research, not forgetting to advocate in season and out of season the truth that research is the essence of the university. If our National Academy of Sciences seems less active and influential than the Paris Academy, this is a reason for taking greater interest in its proceedings. If the American Association for the Advancement of Science does not accomplish as important work as the British Association, this is a reason for attending the present meeting.

Such institutions are essential for science, and those who do not aid in their support are parasites in the body politic. They are essential in part as connecting links between the man of science in his workshop and the intelligent public outside. Investigations require money; this will be forthcoming from the Nation, from the State and from the man of wealth if the needs and importance of science be brought into notice, but not otherwise. More than money, science requires recruits. The best ability of the younger men is needed and should be obtained. We must not depend on in-breeding, but should draw from the widest field.

Our scientific institutions are not only essential in order to keep science in connection with the outside world, but also to hold men of science in touch with each other. Specialization must not be carried so far that the final unity of science is forgotten, and men of science must not lose the stimulus of communication and personal contact. For, as Professor Newcomb wrote in his introduction to the new series of this Journal, "The experience of centuries shows that great successes in advancing scientific knowledge cannot be expected even from the most gifted men so long as they remain isolated."

In order to do the best we can for science in America our duties are many and are often difficult and conflicting. But at the present moment the next step should be in the direction of Buffalo. The decennial meetings in that city have hitherto been successful, both from a scientific and from a social point of view. If all those will attend next week who have at some time proposed to do so, or who would if it were not for relatively slight obstacles, the meeting can be made the most important in the history of the Association.

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*ZOOLOGY AS A FACTOR IN MENTAL CULTURE.**

It is not my purpose at the beginning of this address to weary you with apologies. I

*An address delivered before the department of Natural Science Instruction of the National Educational Association, July 10, 1896.

President David Starr Jordan, of Stanford University, expected to discuss the subject of this paper before the Association, but his absence on the commission to investigate the seals in the Alaskan waters prevented him from preparing the paper and from being present at the meeting. The writer was solicited to fill the gap a few days before the meeting.

which we should seek to advance on terms of equal service.

Atmospheric conditions have led to success in astronomical observations and photography. The great extent and diversified surface of the land have offered unusual opportunities for geological research and have preserved rich paleontological remains. The surviving tribes of savages and the unobliterated relics of extinct races have given the anthropologist a favorable field. In astronomy, in geology, in paleontology and in anthropology we have not failed to take advantage of our position and stand equal at least with other nations. But the extent and newness of our *habitat* bring with them certain corresponding disadvantages. We have no one center, such as London, Paris and Berlin, where men of science may meet and be stimulated by personal contact. We must travel great distances, and at a time of year when traveling is most difficult, in order to attend the annual meetings of our Association.

Our unexplored resources have made desirable, and our more flexible institutions have made possible, the establishment of scientific departments under the government. The Geological Survey, the Coast and Geodetic Survey, the Weather Bureau, the Department of Agriculture and other institutions, have been supported by liberal subsidies and have contributed greatly to the advancement of science. On the other hand, the connection of science with politics is fraught with many dangers, and the alliance requires continual vigilance in order that the liberty of science may be maintained.

The rapid development of our material institutions has stimulated invention and the applications of science. But it is probable that in some cases the energy directed to applied science has been diverted from the advancement of pure science. The acquirement of large fortunes and an aristocracy of wealth have lead to the rich endowment of educational and scientific institutions. On the other hand, the attractions of commercial success have drawn too much of the best ability of the country, and we lack a leisure class contributing to, and taking an intelligent interest in, the progress of science.

Our advantages we have and need not lose. The drawbacks are such as can be obviated or mitigated by proper appreciation and generous effort. Men of science should unite and stand together, even though on occasion it may require self-sacrifice on the part of the individual. In every community, whether of men or of the lower animals, each member must be prepared to sacrifice something, and it may be everything to the general welfare. A community whose members are not ready to give and to take cannot survive.

No one can consider what a difference it would make to the world at the present day if the men of science of this century had not been faithful to their work, without realizing the responsibility of those of us who are now engaged in the advancement of science. Not only our material surroundings, but also our social institutions and ethical ideas, are dependent on the progress of science. Those who appreciate the extent to which this is the case will not willingly leave scientific work for the counting room or patent office; they will not

wish simply to pay my tribute of respect and admiration to the great zoologist and still greater man whom I, with you, hoped to hear this day.

It is with regret that we miss his noble presence and speech, but there is also an element of gratification, for he is the fittest possible representative the government could have chosen as head of the commission to investigate the seals in Alaskan waters, and thus to furnish the definite information upon the basis of which the two foremost nations of the globe can honorably unite in a common cause.

In the able addresses which have preceded there has been shown with great clearness and force how the mind of man, cultivated by the disciplines of physics, chemistry and botany, has been made fitter to yield the flower and fruitage of noble effort. What then has zoology contributed, and what is it likely to contribute when used as one of the agents or means in the cultivation of the mind? And as with the agriculturist, every factor is of interest which can serve in adding to the productiveness of the soil and the quality of what is produced, so to us, mind or soul culturalists, every factor in mind culture is of vital interest. What then is this zoology which is spoken of as a factor in mental culture? As botany in its broad sense includes everything known and knowable concerning plants, so zoology includes everything known and knowable concerning animals; or as botany is plant-biology, so zoology is animal-biology, and deals with the form, structure, activities, development and classification of animals and their economics or relations to each other and to man. And if we include *Homo sapiens* among the animals, it will be seen that if man and his doings are a part of zoology, zoology, like every other center of knowledge and investigation, reaches out to infinity in every direction like the rays from a luminous point.

Although most of us are engaged in the profession whose high aim is to aid in starting the young on the road that leads to a truely liberal culture, it may perhaps be best, before discussing the part which zoology has taken and may take in liberal culture, to understand distinctly what is meant by culture or education, and especially by liberal culture. It seems to me that no one has so well pictured the ideal liberal culture or education, or has realized it more surely in a noble life than the great zoologist, Huxley. Hear his definition: "That man, I think, has a liberal education, who has been so trained in youth that his body is the ready servant of his will, and does with ease and pleasure all the work that, as a mechanism, it is capable of; whose intellect is a clear, cold, logic engine, with all its parts of equal strength, and in smooth working order; ready, like a steam engine, to be turned to any kind of work, and spin the gossamers as well as forge the anchors of the mind; whose mind is stored with a knowledge of the great and fundamental truths of nature, and of the laws of her operations; one who, no stunted ascetic, is full of life and fire, but whose passions are trained to come to heel by a vigorous will, the servant of a tender conscience; who has learned to love all beauty, whether of nature or of art, to hate all vileness and to respect others as himself."

What has zoology done to make such culture possible? First and foremost, it has aided most powerfully to render free the human mind; and without freedom no human soul can enter into the fullness of its kingdom; the true glory of this kingdom is not for slaves.

At the present day no Caesar on the banks of a Rubicon would make his crossing depend upon the omens gained from the flight of birds. We do not decide upon attending the meetings of the National Educational Association by the key in which

the wolf howls or the quaver of the owl's hoot. We no longer expect our acquaintances to imitate the transformations of the companions of Ulysses in the palace of Circé, no matter how appropriate such transformations might be. No longer do we expect to see birds and beasts produced in the fruits of trees or from decayed wood washed by the sea; nor do we think that bees and other insects are generated by decomposing flesh. We know that no living thing exists without having received its life from a living parent like itself. Our path is no longer beset with hippogriff, basilisk or dragon, and our high hopes and noble aspirations are no longer at the mercy of fairies and genii. Living beings, as well as lifeless matter, are subject to law. 'Thus far and no farther,' applies to them as to the waves of the sea or the rush of a comet. The fairies are fled, the genii banished, the mermaid and the remora are captured, classified and harmlessly repose as objects of curiosity or instruction in the great museums. Zoological truth has freed us from their slavery.

Now that freedom has come how shall this subject be made an efficient means of mental culture, and what will its fruit be? In the first place, as for the subjects, the discussion of which has preceded this, Nature herself must be interrogated. The successful student of zoology, to quote again the trenchant words of Huxley, "absolutely refuses to acknowledge authority as such. For him, scepticism is the highest of duties, blind faith the one unpardonable sin. And it cannot be otherwise, for every great advance in natural knowledge has involved the absolute rejection of authority, the cherishing of the keenest scepticism, the annihilation of the spirit of blind faith; and the most ardent votary of science holds his firmest convictions, not because the men he most venerates hold them; not because their verity is testified by portents and wonders; but because his experience teaches him that whenever he chooses to bring these convictions into contact with their primary source, Nature, whenever he thinks fit to test them by appealing to experiment and to observation, Nature will confirm them. The man of science has learned to believe in justification, not by faith, but by verification." To complete this first law in the Decalogue of the scientific student it should be followed by this from his address upon Descartes' Discourse: "When I say that Descartes consecrated doubt, you must remember that it was that sort of doubt which Goethe has called 'the active scepticism, whose whole aim is to conquer itself;' and not that other sort which is born of flippancy and ignorance. "But it is impossible to define what is meant by scientific doubt better than in Descartes' own words. He says: 'For all that, I did not imitate the sceptics, who doubt only for doubting's sake, and pretend to be always undecided; on the contrary, my whole intention was to arrive at certainty, and to dig away the drift and the sand until I reached the rock or the clay beneath.'"

In this spirit, then, of reverent skepticism, of scientific doubt, must the teacher of zoology teach and the student learn. And if this is the spirit, the teachers are but elder brothers a little farther advanced, knowing a few more of the delusions and pitfalls which beset the way. Teacher and pupil work together—the one inspired by the great works of all his predecessors and by Nature herself, and he in turn inspiring and helping the student in his efforts. Such teachers, such pupils and such inspiring surroundings are described by Agassiz in his notable address upon Humboldt: "I was a student at Munich. That university had opened under the most brilliant auspices. Almost every name on the list of professors was also prominent in some department of science or literature. They

were not men who taught from text-books or even read lectures made up of extracts from original works. They were themselves original investigators, daily contributing to the sum of human knowledge * * * and they were not only our teachers but our friends * * *. We were often the companions of their walks, often present at their discussions, and when we met for conversation or to give lectures among ourselves, as we constantly did, our professors were often among our listeners, cheering and stimulating us in all our efforts after independent research. My room was our meeting place —bed room, study, museum, library, lecture room, fencing room—all in one. Students and professors used to call it the little academy ***. It was in our little academy that Döllinger, the great master in physiology and embryology, showed to us, his students, before he had even given them to the scientific world, his wonderful preparations exhibiting the vessels of the villosities of the alimentary canal; and here he taught us the use of the microscope in embryological investigation."

A rare privilege is it, my fellow teachers, to be not only teachers, but friends to our students. For Agassiz, Humboldt and Cuvier were his teachers and friends; for Darwin, were Henslow and Sedgwick. Darwin paid his debt of gratitude by never turning a deaf ear to an inquirer; and in the Origin of Species, the Descent of Man and his other works he becomes a companion to all of us and takes us into his confidence. And Agassiz, what shall we in America not say in gratitude to him! Who like him breathed confidence into the ardent young men who now are bearing the burden and heat of the day in the noble onward march of American science? Who like Agassiz showed us our rich inheritance and inspired this New World to arise and take possession of its own? As in holiness, so in literature, so in science, it is the living gospel, the living teacher whose inspiring touch awakens a spirit that thenceforward can never repose in idleness and indifference, but with a noble enthusiasm ever presses onward.

But, after all, the student comes back in his own mind to the serious personal question: How shall I begin; what can I do to gain this mental culture? Though the practice is difficult, the theory is simple. Observe, study, reflect. But reflection must always follow the others or there will result only empty subtleties, while without reflection observation and study are barren and fruitless. Perhaps it is unnecessary to add that zoological culture does not come from the study of a fourteen weeks' course, prepared by a man who does not know the subject at first hand. Learning the names and a little of the structure and some of the habits of a few animals is not zoological culture, although it may be a beginning. It is such a beginning as learning the Greek alphabet is for the appreciation of the immortal epic of Homer and the whole glorious array of Greek art and literature. Or it is such a beginning as a knowledge of the multiplication table is for mathematics. I have thought sometimes that in our enthusiasm for scientific study we have cut and trimmed and selected for our fourteen weeks' courses till verily when our students ask us for bread we have only a stone to offer.

Did Darwin think out natural selection and the survival of the fittest or Agassiz the glacial theory in fourteen weeks? Not every pupil can spend 28 years or even a tenth of that upon a single subject; it nevertheless remains true that the mental culture gained by the study of zoology will, as with other disciplines, depend *first upon the original power of the student *and second upon the time and energy devoted to the subject.*

*The original ability of the student is mentioned prominently in this paper because, in too many discus-

If we take some of the aspects under which zoology may be considered, as anatomy, physiology, embryology, classification and economics, and think for a moment what is involved in understanding them, perhaps it will be clear why it is so insisted upon that to gain true mental culture from zoology time is required. Time for observation and study, and, after that, time for reflection, so that there may be assimilation and some kind of real comprehension of the subjects considered. And I take it that in the comprehension gained lies the very pith and marrow of whatever culture zoology can give.†

If anatomy is considered, what a field is there for observation and study. This animal machine with its muscles and nerves, digestive system and brain, bones and sinews; what nice adaptations they show for their various purposes, and to the far seeing eye how many bungles and compromises there are too. As compared with the machines made by human hands the animal machine is as a printed volume to a simple diagram. In these archives are stored the history of the past, the ascent or the descent from something different, but like the manuscript that has been written over and over after partial erasure, so is this structure clear only in part. Some words have been spelled out, but the master to decipher the whole manuscript is yet to appear.

And physiology, that is, the activities of the living animal, how beautiful they are, how diverse. The mother love that saves the world, the mighty thought of Newton or Shakespeare are somehow bound up with or in this living matter whose chemistry and physics even, still almost wholly elude us.

Then if we turn to embryology and try to trace with patient care the work of the unseen artificer who arranges the apparently simple and almost structureless mass of the ovum into heart and brain, muscle and nerve, and changes the formless into forms of beauty and power, be it butterfly, bird or man, we cannot but receive culture and uplifting; for are we not seeing with

sions upon subjects for culture, teachers and methods, it seems to be assumed that, given a proper subject of study, a good method and an expert teacher, the desired result will be attained. That is, the material upon which the teacher works is tacitly left out of the count, and the teacher is blamed or the method or subject is condemned if cultured men and women are not turned out regardless of their ability. It is a historical fact, however, that with good or poor teachers or with no teachers, with good or poor methods or apparently with no methods, and with a great variety of subjects, cultured men and women have appeared in all ages. Subject, method and teacher are only helps that the student uses according to his ability, and important as the helps are, the result depends infinitely more upon the native ability of the student than upon the helps. Subject, method and teacher cannot create they can only modify or facilitate development.

†It is not for a moment claimed that so thorough a study of zoology as is here advocated is the only way to obtain *useful* information concerning the animals upon the earth and in the water. To continue the comparison used in the text, a little knowledge of Greek is useful in studying astronomy, and for gaining a better appreciation of English words derived from the Greek, but no one claims that such elementary knowledge is Greek culture. So information concerning edible fishes, mollusks and the ordinary four-footed creatures, a knowledge of poisonous snakes, useful and harmful insects, and many other practical and useful things, may be known about the animals, but that is not the knowledge that makes culture, although the profounder knowledge advocated in this paper and which comes with culture in zoological science includes this which in itself is merely practical and useful. Real science or culture gives foundation principles which alone make applied or useful knowledge possible in the higher fields. While I believe most thoroughly that zoology for culture is a very serious subject and one requiring much time as well as much observation and reflection, it is not desired for a moment to discourage the study of zoology, or indeed any subject, for purely utilitarian or practical purposes. While indeed such knowledge cannot be called culture, it is often true, as aptly stated by Prof. Atkinson in discussing this series of papers, that study for purely utilitarian purposes is very likely to lead to the higher kind of study which does make for culture.

our own eyes what is described in the sublime words of the Psalmist: "I am fearfully and wonderfully made * * *. My substance was not hid from thee when I was made in secret and curiously wrought in the lowest parts of the earth. Thine eyes did see my substance, yet being unperfect; and in thy book all my members are written, which in continuance were fashioned when as yet there was none of them."

Classification requires knowledge of all the above, for it is an arrangement in due order of the complex beings of the earth from the microscopic animalcule to the mighty elephant. For the classification to be successful the mind must see the true relations between all the forms, must know their structure and activities and how they were curiously wrought and transformed from generation to generation for unnumbered ages; in a word, the classifier must know their evolution; or, in the noble words of Agassiz, he must 'become the translator of the thoughts of God.'

And lastly we come to the economics of zoology, that is, the relations of the animals to the earth, the plants, to one another and to man, and his relations to them. Here one is brought face to face, not merely with the glory of living, thinking and acting, but with destiny; with the solemn fact of life *with* death, or, more truly stated, life *by* death. More are born than can possibly survive even the short span granted for the typical life cycle. Indeed, it almost appears as if nature in her efforts for life had become a Moloch of death. How graphically Darwin has painted the picture of this scene of strife, the plant crowding its neighbors to get a little more sunshine or nutriment, the animals crowding each other and devouring both the plants and their fellows; and then there is the whole foul brood of animal parasites. In these latter days we know also that the plants are not simply content to strive for sunshine and soil in order to elaborate from the inorganic world the compounds that alone make animal life possible, but in turn, a multitude of them, which no man can number, the bacteria, are devouring the animals, including man. The knowledge of this fact, so largely due to the great Pasteur, has given new significance to hygiene and a new meaning to cleanliness.

This death and disease of the animals by means of the pathogenic germs, which also bring disease and death to man, has put a new aspect upon man's relations with the animals. They are indeed his kin, and *zoological economics* may almost be said to have become dignified into *zoological ethics*. None stands or falls alone. The earth is the mother of us all, but she bestows her gifts in a very roundabout fashion sometimes. The soil, air and sunshine of Montana may furnish the conditions for the grass; the old world gave the foundations of the life which we now find realized in perfect form in the sturdy beeves which grow and fatten on the Montana grass; and finally, without a thought of the sun, or the soil of Montana, or of the life which they made possible, or of the fear and suffering which may have resulted, we calmly nourish ourselves on the beefsteak while discussing politics, education or the hereafter. But often enough to take away undue indifference, the beef or other food may contain the germs of what is death to us, although it may be teeming life to the germs; and there is forced upon us a consideration or our relations with our living environment. If knowledge and reflection are sufficient, it does not take a very great philosopher to see that the economical standpoint changes with the change of organism. For the plant, the sunshine, the soil and the rain are for it. For the plant-eating animal, sunshine, soil and rain are to produce the plant for it. And from man's standpoint, all are for him; but if we change the stand-

point slightly and judge of the workings of a tiger's mind by its actions, we would see that sunshine, soil, rain and dew, the plants, the fat beeves and even man himself are for the tiger's sole benefit.

Surely if the other sides of zoology call for imagination, acute observation, profound study and cold, logical reasoning for their comprehension, this side demands all these and in addition a philosophic spirit, that flower of the cultivated human mind.

I think what has been said will suffice to show that in zoology there is a factor of true mental culture; and that by it the philosopher, the philanthropist, the man of affairs, is better fitted in his own sphere for work and for leisure. If the student feels that some of the inspiration to this culture has departed, that the structure, function, embryology, classification and economics of animals have been almost all discovered and determined, and may be found in the ponderous volumes and monographs in the great libraries, refer him to Aristotle, Darwin, Dana, Gray or Agassiz, or to any of the devoted men and women who have been and are trying to find out the truth and to follow it, they will say: Be of good cheer, and not faint hearted. Look and listen with *brain* as well as eye and ear, for on every side are thrilling sounds whose music no human ear hath heard, and sights whose exquisite beauty no human eye hath seen.

In closing this address I cannot summarize my belief in the cultivating power of the earnest study of zoology better than by saying that a profound contemplation of the factors in the problem of animal life on the earth will bring out and cultivate the mind. It will show man his true relations to his fellow men and to his lowly fellows, the animals. It will not fill the mind with pride, for ultimate knowledge is as yet unattainable; it will rather give the humility expressed by Job: "Canst thou by searching find out God? canst thou find out the Almighty unto perfection?" or by Newton: "I do not know what I may appear to the world; but to myself I seem to have been only a boy playing on the seashore and diverting myself in finding now and then a smoother pebble or a prettier shell than ordinary, whilst the great ocean of truth lay all undiscovered before me." And another from one of the foremost physicists of our own day, Sir William Thompson, at the jubilee of his appointment as professor of natural philosophy at the University of Glasgow: "One word characterizes the most strenuous efforts for the advancement of science that I have made perseveringly through 55 years; that word is failure; I know no more of electric and magnetic force, or of the relations between ether, electricity and ponderable matter, or of chemical affinity, than I knew and tried to teach my students of natural philosophy 50 years ago in my first session as professor." Yet there is also the pean, if not of victory, of the consciousness of power that comes to him whose mind has been truly cultured by the disciplines brought before you in this series of addresses and none has a surer right to that consciousness or with a surer voice has expressed it than the zoologist in whose place I stand to-day: "The world of thought and the world of action are one in essence. In both truth is strength, and folly and selfishness are weakness. Say what we may about the limitations of the life of man, they are largely self limitations. Hemmed in is human life by the force of the fates; but the will of man is one of the fates, and can take its place by the side of the rest of them."

Simon Henry Gage.
Cornell University.

INSTINCT AND EDUCATION IN BIRDS.

The discussion, first provoked by the note in Science of February 14th relative

to the origin of instinct and the inheritance of acquired habitual actions, and the remark of Prof. Wesley Mills (p. 441) that "before drawing conclusions from observations on domestic animals it is well to consider similar facts in connection with their wild congeners," have led me to make a few experiments upon a fledgling of our common kingbird (*Tyrannus tyrannus*), captured July 2d, as it was taking one of its first lessons in flight.

As is well known, the kingbird is exclusively insectivorous and generally captures its prey on the wing, though it does not refuse insects that may lurk in the foliage, and it may occasionally descend to the ground in pursuit of grasshoppers, whose movements have betrayed their whereabouts. Being thus in its activities so different from the omnivorous chick, and belonging, moreover, to the great group of Gymnopædes, or birds, which, naked-born, are fed in the nest, we might expect certain differences from the instincts and habits of the precocious, downy chick. Such differences may throw light upon the questions of comparative psychology though, as the material for purposes of generalization is augmented, they may prove to be variations of no direct suggestive value.

From July 2d to the 11th the bird, almost incessantly calling for food, was kept in the house and fed, from the hand, shreds of meat, moist bread and a few insects. Water was taken from the wet finger, not as a drop from the tip, but finger and all were seized, the subsequent motions of deglutition being the same as though any large morsel were being engulfed. To the present day (July 16th) the bird has utterly refused to accept the pendant drop; nor could it be induced to peck a drop from a leaf or from the surface of any object whatever.

On July 11th I offered the bird a small porcelain dish (such as is used for extract of beef) filled with water. Though hungry and presumably thirsty, no effort was made towards taking the water, but the dish was repeatedly seized with the same eager fluttering that characterized the general reception of any proffered article, edible or not. (It was noted that the tongue during this act was in rapid motion.) While making an unusually awkward lunge at the edge of the dish the bill was accidentally thrust deep into the water, and quickly withdrawn with an unmistakable air of surprise, followed by an effort to *eat* the water held between the mandibles. The jaws snapped; the tongue could be seen shooting back and forth, and the head, first held horizontally, was only slowly tipped backward and then, not in the way of the chick, described as instinctively perfect, but after the retching method of mouthing and swallowing any object not readily responsive to the contractions of deglutition and which must needs have the added assistance of the attraction of gravitation.

Though the porcelain dish was afterwards repeatedly offered from July 11th to the 16th and invariably evoked notes of approval, the bird in securing the liquid always bit the edge and never once dipped the beak beneath the surface, nor drank in the approved method of the chick. The earlier awkward movements, however, were greatly improved through repetition. The substance of the water seemed never to be visually observed, and the empty dish held in the hand evoked the same clamorous approval as when filled with water, and was later recognized even when accidentally met, though a saucer which had not contained food or water evoked no sign of interest.

On the morning of July 12th it was noted that if water was allowed to fall from a height the bird became greatly agitated, opened its mouth and vigorously struck at the descending drops, and several were

swallowed with evident signs of relish. Up to this time, while in my possession, the animal had taken food only when placed by the fingers in the gaping mouth, and had made no effort to pick, selectively, the food from between the fingers ; nor had it even changed its position on the approach of food, but had remained in one place, fluttering and incessantly calling until the food was brought to it. On the morning of the following day falling drops were again struck at and seized, though the bird did not relish the accompanying wetting. At noon the drops were again seized and swallowed. Signs of disapproval of the wetting were shown on the morning of the 14th, and on the morning of the 15th the bird avoided falling water and was content with biting the edge of the dish.

From the above observations I am inclined to agree with Prof. Mills that the nature of eating and of drinking are not radically different and, as the physical condition of substances may pass imperceptibly from solid to liquid, so the physiological processes are practically the same whether the food is solid, pultaceous or liquid ; though I should not attempt to compare too closely the relative perfection of the two processes (p. 356). I do not, moreover, feel that the first act of drinking is in its totality necessarily instinctive (p. 355). In other words, 'when a chick first drinks on its beak being put into water' the act may be considered as, very largely, a result of self-teaching.

The phenomena of eating and of drinking have not, in the discussion, been definitely defined, and there has been some lack of discrimination in the use of the word 'swallow.' The beak, moreover, is mentioned by Prof. Mills and Lloyd Morgan, as the organ the stimulation of which produces the act of drinking, though Prof. Baldwin attributes the action to the stimulation of the sense of taste.

It seems to the writer that the entire process of eating and drinking should be divided into three parts, viz., (1) seizure, (2) mouthing or mulling and (3) deglutition. It is only in the first of these that the term instinct in the sense of inherited habit is necessarily used. Baldwin, Mills and Lloyd Morgan are practically agreed that the young chick seizes instinctively on being stimulated by some small, striking object at a suitable distance. This object may be nutritious or it may be a feather, a pencil or a nail head, a drop of water or a drop of ink. The mechanism is ready and the stimulus properly applied produces the instinctive mechanical, or, as Lloyd Morgan would prefer, organic action.

The object now held between the mandibles and mulled is subject to the examination, strikingly evident in the kingbird, of the tongue, an organ at the same time tactile, gustatory and locomotory. It stands at the portal which leads from instinctive to reflex action and is at once the inspector, reporter and director of that which first stimulated the eye and now, through a motor response, has been placed where it may stimulate other special sense organs : taste, touch and probably smell. It is here that instinctive action becomes guided by individual control, and intelligence begins to act through experience.

The mouth-parts of the young kingbird are large and the deliberate movements are easily observed. I feel therefore that this second and essential portion of the process of eating and drinking in the small-mouthed chick may have been neglected or overlooked. Moreover, the process of the perfecting of the action of eating and drinking through repetition and the guidance of the intelligence is, in the kingbird, comparatively slow and inclines one on the grounds of comparative psychology, to the belief that the complex act of the chick may be only *apparently* perfect from the first, the

successive processes of coordination being in the chick much more quickly perfected.

The process in the kingbird as above detailed gives at least an opportunity for the more definite limitations of those actions which Prof. Baldwin has, perhaps unfortunately, called half-congenital.

The action of the callow bird in deglutition is probably performed as a reflex on the stimulation of the presence of food in the pharynx. Small fragments upon the beak and in the anterior portion of the mouth are not perceived and do not quiet the almost irritating clamor of the gaping young. The enormous size of the mouth, the thickened 'lips' and the bright colored concentric markings of the oral walls make a target, the sensitive center of which (the opening of the œsophagus) only a most awkward parent could fail to hit. We might argue that the young nestling has not, at first, a definite sense of taste, and actual experiment on the kingbird shows that most unsavory morsels when placed in the mouth are swallowed, though not without subsequent signs of surprise, if not of disgust. It is not, then, difficult to perceive that the young bird while still within the nest acquires, as a result of the selective activity of the parent, a taste for certain food. The discriminative exercise of the sense of taste is thus a result of direct tuition. The young cow-bird whose foster parent has been a vireo will doubtless acquire a relish for food very different from that enjoyed by, perchance its own brother, but, the ward of a graminivorous finch.

It may be objected that the orphan chick selecting food without the discriminative direction of a parent, is not a parallel case with the young kingbird: The bird in my poseession was so tame that when it reached an age comparable with the newly-hatched chick, I could take it into the fields and observe it as it foraged, chick-fashion, for itself. I think that I saw it capture its first insect; I, at least, observed its ability as an insect catcher develop from almost *nil* to expertness. During these excursions observations were made and data collected for the determination of the following questions: Is there an inherited discrimination in favor of the capture of certain edible insects in preference to others? If unsavory insects are unwittingly taken into the mouth are they swallowed? If ejected from the mouth are there signs of disgust? When unsavory examples are met a second time are they avoided?

To the first question I can reply that, at first, all insects were indiscriminately seized. A vile-smelling Hemipteron was as tempting as a luscious grasshopper or cricket. Distinctly unsavory insects (*Tetraopes*, *Coccinella*) were not touched a second time, except with the greatest caution; though species which were only moderately distasteful (*Lema*) might be taken and devoured, but *without* relish. In one case a large brown ant, the first found, was seized, mulled and vigorously ejected. The next day the bird was taken to the same tree and, on perceiving a second ant of the same species, eyed it closely and deliberately, and then shook its head and vigorously wiped its beak with unmistakable signs of recollection. I mention this particular case, though it is not the only one, to illustrate how quickly the bird was self-taught, for the ant was only one of a dozen different species of insects which were met, and it was so instantly seized that a prolonged visual image was not gained. I might add that the kingbird subsequently refused even to try the edible qualities of a large black ant of a different species, though the bird watched the insect's movements with much interest. Profiting by mistakes it soon learned to examine critically all strange food before the tongue should force the contents of the mouth on towards the pharynx.

Can we not then conclude that the forcing of acceptable food and drink into the pharynx is not 'instinctive,' but is the result of a series of satisfactory discoveries of the young bird which lead up to the placing of the food where it will bring about the stimulation of the reflex center of the gullet and the accomplishment of the final act of swallowing—a series which is intelligently adopted by the bird and improved by practice.

It is perhaps well, before closing, to revert to the peculiar habit of the bird in snapping at falling drops. From the first, the attention was markedly attracted by flying insects and any small objects in motion seemed to have a peculiar charm. From this fact I am inclined to think that the seizing of drops was no more than the striking at moving objects, though it is possible that the adult habitually takes water on the wing by seizing falling drops of dew or rain. H. C. BUMPUS.

A NORTHERN MICHIGAN BASELEVEL.

KEWEENAW POINT and its southwestern extension in northern Michigan is composed of rocks of Keweenawan and Cambrian ages, and exhibits three chief topographic features. Beginning at the south is a broad area of the so-called Eastern or Potsdam sandstone. This is in a horizontal position, and rests unconformably upon the rocks to the north and south. North of this area is the main trap range of the Keweenawan, which consists largely of basic lava flows, but with lesser quantities of acidic lavas. Interstratified with the lavas are numerous layers of sandstone and conglomerate. The majority of these are thin, but in the upper parts of the series some of the conglomerates are of considerable thickness. The breadth of the main trap range varies from about 4 miles to nearly 10 miles. In a general way the traps and detritals strike northeast and southwest, and dip to the northwest at angles varying from 25° to 55°. At the southwestern part of the area considered, a wing of the trap range swings to the north as the result of a fold. This area is known as the Porcupine mountains. The distance from the southwest part of the Porcupine mountains to the end of Keweenaw Point is about 120 miles. To the northwest, overlying conformably the main trap range, is the upper division of the Keweenaw series, which consists wholly of conglomerates and sandstones. The dips on its southeastern border average about 25°, but they become less and less toward Lake Superior and at the shore they do not average more than 8° or 10°.

For a full description of the Keweenaw series see the Copper-bearing Rocks of Lake Superior, by Roland D. Irving, Monograph V., United States Geological Survey, and in connection with the present description see the maps of plates I., XVII., and XIX.

A recent visit to this area convinced me that this district had been almost completely baseleveled. The two most advantageous points found by me from which this baseleveled area may be seen are, first, the top of the hill occupied by a church in the village of Rockland, and, second, the top of the rockhouse of the Quincy mine, occupying the highest ground above Hancock. From the Rockland point, looking to the northeast the main trap range appears to be an almost level plain. To the southwest the plain is nearly as level, but the Porcupine mountains rise considerably above this plain. The explanation of this monadnock is simple; the core of the Porcupine mountains is hard quartz-porphyry and felsite, rocks more resistant than the interstratified traps and detrital rocks of the main trap range. From the Quincy rockhouse on a clear day the eye sweeps from the Porcupine mountains on the southwest to the end of Keweenaw Point, to the northeast, that is, over the entire 120

miles, and to the northeast Isle Royale may been seen. Again ignoring the immediate foreground, there is an impression of almost absolute horizontality, with the exception of the Porcupine mountain mass, to the southwest, and of some peaks on the south side of the main range far out toward the end of Keweenaw Point to the northeast. These northeastern monadnocks are supposed to be parts of the Gratiot Bluff, Mount Bohemia, and Mount Houghton range, which is known to consist of hard quartz-porphyry and felsite.

The immediate foreground both at Rockland and at the Quincy mine is exceedingly rough, and these two places are typical of the range. When traversed the range is found to be cut by steep ravines, to be carved into bluffs and hills, and everywhere one is ascending or descending. The apparent plain is evidently composed of the higher points of the range, which rise just about to the altitude of the ancient baselevel. Scarcely a remnant of the plateau which once must have existed is left. As determined from the topographic map of the outer part of Keweenaw Point, made by the United States Lake Survey, the Lake Superior baseleveled area is at an elevation of about 1350 feet. The culminating points from near Eagle Harbor east run as follows: 1349, 1344, 1292, 1330, 1312, 1335, 1330 feet. The last is East Bluff, and this is only about 4 miles from the end of the Point. Upon account of the northwest dips these and most of the other bluffs of the district have comparatively gentle slopes in that direction and steep slopes to the southeast where the layers are broken across. Such a remarkable uniformity as given above in the height of peaks carved from tilted rocks of varying hardness could not be the result of erosion of an elevated area. The only satisfactory explanation yet offered for such phenomena is the standard one of an elevated, baseleveled plain which is undergoing a second cycle of degradation. The culminating points of the south part of the range which rise above the baseleveled area are as follows: A point about 3 miles south of west from Gratiot Bluff, 1534 feet; Gratiot Bluff, 1435 feet; Mount Bohemia, 1469 feet; Mount Houghton, 1429 feet. These points therefore rise from 100 to nearly 200 feet above the baseleveled area. According to Irving, many of the ridges of the Porcupine mountains have elevations of 1600 to 1800 feet, while but small parts of the lower portions are as low as 1400 feet. This places the higher points of the Porcupine mountains from 250 to 450 feet above the baselevel.

For the greater part of the area the present cycle of erosion is evidently at its full maturity, and for the outer part of the Point, from which the altitudes above given are taken, just past that stage.

The Potsdam sandstone to the southeast and the upper division of the Keweenawan to the northwest of the main trap range, on account of their softness, are everywhere cut below the ancient baseleveled plain. These areas, unlike the trap range, are in large part so nearly reduced to the level of Lake Superior as to show very much less irregularity of surface than the main trap range. Although cut by river valleys and ravines, and although there are slopes everywhere and in many places very considerable irregularities of surface, the comparative flatness, as contrasted with the Keweenawan rocks of the main trap range or with the Huronian and Archean rocks to the south, is very marked.

A cursory glance at the maps of the district in question shows that when topographic maps of the entire area are made, and its drainage studied, numerous interesting features will probably appear. The trap range is traversed by the stonger streams, such as the Presqu' Isle, the Ontonagon, the Fire Steel and Flint Steel rivers, and Portage

Lake, but the majority of the smaller streams flows to the north and to the south from the central trap area, and they are thus consequent.

The west branch of the Ontonagon river flows from Agogebic lake along the southeast border of the trap range for 20 miles or more. Here it joins the other main branch and the river breaks directly across the trap range in a southwesterly direction.

The only transverse cut where the trap range is reduced almost to the level of Lake Superior is at Portage Lake, and this place has been utilized for a ship canal. From Portage Lake the banks rise steeply from 500 to 700 feet, nearly to the baselevel above. No explanation for this exceptional reduction has been offered. One is tempted to believe that here must have been unusual fracturing or faulting, and this idea is encouraged by the presence adjacent to Portage Lake of a number of important copper mines on the amygdaloids. It is well known that the amygdaloid mines occur where there has been much crushing of the porous rocks, as a result of the differential movement between the layers of trap. A partial explanation of the Portage Lake gap may be the comparative narrowness of the range at this point, as a result of steeper dips. This increased steepness of dip implies greater accommodation between the layers, and therefore more fracturing of the rocks.

The Little Montreal river, which rises on the trap range, flows in a nearly east and west course for 15 miles in one of the softer divisions of the Keweenawan series between two harder divisions, before turning abruptly to the south and breaking through the porphyries, felsites, and traps. Had it continued four miles farther, in a course little south of east, it might have reached Lake Superior at the end of Keweenaw Point without breaking through the resistant formations. A close examination of Irving's Plate XVII. of Monograph V. referred to shows many other interesting points in reference to the drainage.

In a recent number of Science, July 17, 1896, I described a central Wisconsin baseleveled area, more nearly perfect than that at Keweenaw Point. From center to center of the two districts is about 150 miles. The central Wisconsin district has not been so deeply dissected as Keweenaw Point, but this is readily explained by the fact that it is not so near either of the Great Lakes, and therefore erosion has not so thoroughly stretched its fingers over it. The central Wisconsin baseleveled plain is at an altitude of about 1450 feet. The Keweenawan baseleveled plain is at an altitude of about 1350 feet. Therefore the baseleveled areas of the two districts are probably but parts of a far more extensive baseleveled region.

The area intervening between the two districts consists of Huronian and Archean rocks. Resistant quartzites and mica-schists are characteristic rocks of the Huronian, and gneissoid granite is the dominant rock of the Archean. Since the most resistant rocks were not reduced to the sea level at Keweenaw Point or in central Wisconsin, one would expect that the more widespread, equally resistant rocks of the Huronian and Archean would also project above the baseleveled plain. As a matter of fact topographic maps of the Marquette and Penokee districts made by the U. S. Geological Survey, and under my own direction of the Michigamme district southwest of the Marquette district, show that extensive tracts of country are at altitudes from 1600 to 1800 feet or more, thus verifying the expectation. The variations of level of the ancient uneven plain throughout the region, however, are probably not so great but that Davis's term peneplain may not properly be applied to it. So far as I know, H. L. Smyth was the first to call

attention to this fact for the Michigamme district.

In my previous article I suggested that the period of this ancient denudation was Cretaceous, and gave reasons for the belief that the predominating agent in the process was sub-aërial erosion.

C. R. VAN HISE.

CURRENT NOTES ON PHYSIOGRAPHY.

SAN FRANCISCO PENINSULA.

THE geology of the San Francisco peninsula by Lawson (16th Ann. Rep. U. S. G. S.) closes with a chapter on its geomorphy, in which it is shown that two fault blocks—San Bruno and Montara, the first more carved than the second—dominate the form of the region. The bounding faults trend northwest, and the fault scarps faced southwest. After faulting and well advanced dissection, a progressive emergence of the two blocks in unison revealed marine terraces at various levels on their flanks. Recently a slight submergence has drowned the lower stretches of the valleys, the Golden Gate being then made a waterway. An effective colored relief map, photographed from a model, brings out the topography very clearly.

TURKEY LAKE, INDIANA.

A BIOLOGICAL study of Turkey lake, Indiana, under the direction of C. H. Eigenmann, of the Indiana University Biological Station, gives many details concerning outline, depth and temperature (Proc. Indiana Acad. Sci., V., 1895) that may serve as typical for the smaller morainic lakes of the prairie States. For dimensions the surface is five and a-half long by about a mile wide, with a perimeter of over twenty miles and an area of 5.66 square miles. Soundings have shown the bottom to be of rolling morainic form, like the adjacent county. The greatest depth is nearly 70 feet; the average depth, 17. Small natural changes have occurred in depth or outline, except for the conversion of shallow marginal water into swamps. The catchment basin being small, it is estimated that only seven inches of water are drained off through the outlet, while thirty inches pass away by evaporation. The action of ice in forming beaches is described, following Russell.

GEOLOGY AND SCENERY OF SUTHERLAND.

THIS attractive little book of a hundred pages by H. M. Cadell, now appears in a second edition (Edinburgh, Douglas, 1896) and gives us northern Scotland in a nutshell. Although chiefly occupied with geological structure and succession, and with diagram and experimental illustration of the 'secret of the highlands,' due attention is given to the topographic forms characteristic of each formation. The bold mountains of nearly horizontal Torridon sandstone, of which the superb Suilven is among the most striking, are benched and cliffed around by the harder layers, and seem to bear witness to the long undisturbed attitude of these ancient strata; but they are neatly shown to have recovered from a tilted position into which they were thrown in pre-Cambrian times. Eight page plates, a dozen figures, an orographical and a geological map illustrate the text.

GEOGRAPHY IN THE ENGLISH UNIVERSITIES.

SIR CLEMENTS MARKHAM, in his recent annual address to the Royal Geographical Society, announces that the geographical readership at Oxford, subsidized for ten years past by the Society, will be continued by the University without outside aid; the position still being held by Mackinder. Oldham, at Cambridge, has a less assured position, the subsidy there being still continued. Herbertson, at Manchester, is not mentioned, as the Society has not given a subsidy there. It is proposed that a London School of Geography of university rank should be formed under the

auspices of the Society. In comment, it may be said if Great Britain to-day supplies the most active explorers and holds the greatest colonial possessions in the world in spite of the lack of instruction in geography so generally complained of, what will she become when this branch of instruction is duly organized!

THE PAMIRS.

AN entertaining narrative of exploration by Curzon over the Pamirs to the source of the Oxus (London Geogr. Jour. July, Aug.) discusses the meaning of Pamir, discarding the 'roof of the world' as fantastic, and concluding, with much appearance of truth, that a Pamir is an elevated valley (12,000–14,000 ft.), floored with broad slopes of waste from the adjoining lofty mountains (20,000 ft.+), drained by a medial stream, which runs noisily over a stony bed, meanders through a peaty tract or spreads in a lake; buried in snow for seven winter months, but affording abundance of summer pasturage, although devoid of trees and cultivation. The further statements that the Pamir is 'a mountain valley of glacial formation,' and that the inability of the medial stream to scour for itself a deeper channel is due to the 'width of the valleys and the consequent absence of glaciers on any scale' seem to be open to question. Eight different Pamirs are described and mapped.

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CURRENT NOTES ON ANTHROPOLOGY.

THE SIGNIFICANCE OF THE METOPIC SUTURE.

A WELL studied memoir on this subject is that of Dr. G. Papillault, published by the Anthropological Society of Paris (La Suture Métropique, et ses Rapports avec la Morphologie Cranienne, pp. 122. Paris, 1896). His results are derived from a comparison of a long series of skulls of different ages, sexes and races. They clearly indicate that the presence and persistence of the metopic suture is an evidence of superior mental (cerebral) activity and superiority, because this persistence unquestionably finds its point of departure in the brain itself. It is seen most frequently in women and children, and is not uncommon in the Negritos. These facts, however, do not impair the author's position. The superiority he refers to is relative to weight and height, and in that sense he claims that the brain of the female and the child does rank above that of the adult man.

He does not affirm the intellectual superiority of metopics in an absolute sense, but that the capacity and functional energy of their brains are greater in proportion to the whole body than in others. Moreover, he very pertinently adds that nothing is more difficult than to pronounce positively on the intelligence of a race or an individual. Civilization and success are not sure criteria, as every one must admit. The demonstration of his position is ably argued.

THE SVASTIKA AND THE TRISKELES.

IT is singular to how many possible origins these famous symbols lend themselves. The latest is proposed by the well-known explorer, Karl von den Steinen, in a paper contributed to the Bastian Memorial Volume. He believes that the svastika was developed from the conventional outline of the stork, and the triskeles from that of the domestic fowl! He brings forward considerable learning and ingenuity to demonstrate his thesis, and succeeds in rendering it as plausible as a dozen other hypotheses which have been advanced. How the svastika came to be in America, where we have no storks, he fails to explain; in fact, does not refer to the American examples of these figures, which for an Americanist, *ex professo*, is an unexpected oversight. At the close he makes some observations on the Runic alphabets, which he believes are something more than modifications of Latin letters.

The Bastian Memorial Volume contains several articles of interest to students of American ethnography; as Dr. Boas, on secret societies among the Kwakiutl Indians; Seler, on the ruins of Quiengola; Dieseldorf, on the Toltecs; Ehrenreich, on the Botocudo language, etc. D. G. BRINTON.

UNIVERSITY OF PENNSYLVANIA.

SCIENTIFIC NOTES AND NEWS.

IN the issue of this JOURNAL for June 19th, will be found full details regarding the present meeting of the American Association for the Advancement of Science at Buffalo. It will be remembered that the first meeting of the Council will be at noon on Saturday, August 22d, and the first general meeting will be on Monday, August 24th. The work of the sessions has been made continuous, not being interrupted by Sunday or by a day for excursions. The affiliated societies, however, meet on the 21st and the 22d, and all who are able should be present at Buffalo on those days.

PROF. HUBERT A. NEWTON, of Yale University, died in New Haven on the night of August 12th. We hope to give in a future number some details regarding Prof. Newton's life and his important contributions to astronomy and mathematics. Yale University and American science have recently suffered most severe losses in the deaths of Professors Whitney, Dana, Eaton and Newton.

ADVICES by cablegram regarding the eclipse expeditions state that Prof. Schaeberle and Prof. Todd in northern Japan were unsuccessful or only partially successful in their observations, but that the weather was clear and fine in Norway.

CABLEGRAMS to the daily papers report that Dr. Nansen has arrived at Vardo Island, Norway, on board the steamer Windward, which recently went to Franz Josef Land in order to bring back the Jackson-Harmsworth expedition. It is stated that with Lieutenant Hansen he left the steamer Fram, in which he had hoped to be carried to the pole, in March, 1895, in Lat. 84, Long. 10.27, to explore north of the Franz Josef route. The party journeyed through an unknown polar sea, and explored north of Franz Josef Land to Lat. 86.14. No land was seen to the north of Lat. 82; only ice. They wintered on Franz Josef Land, living on whale meat and bear meat. The Fram was reported to be a good ice ship, and will arrive later this year. There was no sickness on board. Although the expedition failed to reach the object of its search, it got four degrees farther north than did any previous expedition.

THE second International Congress of Applied Chemistry was opened at the Sorbonne, Paris, on July 27th, about 1,600 delegates being present. The admirable address of the President, M. Berthelot, is published in the *Revue Scientifique* of August 1.

THE sixty-fourth annual meeting of the British Medical Association was opened at Carlisle on Tuesday, July 28th, under the Presidency of Dr. Henry Barnes, physician to the Cumberland Infirmary. As compared with the meeting in London last year the attendance was not large, about 700 members being present, but the addresses by the Presidents of the sections and the proceedings of the sections contain much that is of interest, not only to the medical profession, but also to those engaged in advancing medicine as a science. Following the British Association for the Advancement of Science, the Medical Association will meet next year in Canada, having accepted the invitation of the Montreal branch. Prof. T. G. Roderick, professor of surgery in McGill University, will be President. The Association will probably meet in Edinburgh in 1898.

THE sixty-eighth meeting of the German naturalists and physicians will be held at Frankfort from the 21st to the 26th of September under the Presidency of Prof. H. von Ziemssen. Lectures before the general sessions will be given by Drs. Hans Buchner, Richard Lepsius, Max Verworn, Ernst Below and Karl Weigert. Before the medical sections there will be a discussion of the results of recent investigations of the brain led by Professors Paul Flechsig, Ludwig Edinger and E. von Bergmann. American men of science will be welcomed at the meeting. Membership cards can be ob-

tained from the Secretary, Dr. Hugo Metzler, Frankfort, on payment of M. 15.

SECTION C., Chemistry, of the A. A. A. S., has printed in advance a provisional program, which shows that as usual the papers presented before this section will be numerous and important. Physical, inorganic and organic chemistry will be taken up on August 25th; didactic, analytical and technical chemistry on August 26th; technical and sanitary chemistry on August 27th; agricultural and biological chemistry on August 28th. More than sixty papers have been already promised.

THE admirable leadership of the department of natural science instruction of the National Educational Association is shown by the addresses given before the recent meeting and published in this JOURNAL. We learn from the President, Prof. Charles S. Palmer, of the University of Colorado, that it is proposed to secure a committee at the approaching meeting of the American Association for the Advancement of Science, and also from the several college associations which will cooperate with the committee from the National Educational Association in discussing the position of natural science in the schools and the establishment of natural science requirements for admission to college.

IT seems probable that owing to contrary winds M. Andrée has not undertaken to launch his balloon. Reports that the balloon has been seen in the Arctic regions are not likely to be very reliable, as the natives have been promised rewards for bringing news of the explorers.

HERR OTTO LILLIENTHAL, while experimenting with his flying mechanism on August 11th, met with an accident which resulted in his death. It is reported by cablegram that he started from a hilltop at Rhinow, near Berlin, and after floating for some distance the apparatus got out of order, causing him to fall to the ground. Herr Lillienthal was 46 years of age.

A PARTY under the charge of Mr. W. G. Miller, of the School of Mining, Kingston, Ont., will leave that place on September 1st to explore the chief mineral localities of the Kingston district. Though primarily intended for students of the school, we understand that others would be admitted to the party. Travelling will be done by canoes, and it is estimated that the expense of the trip will not exceed $20 for three weeks.

ON the afternoon of July 26th there was a heavy hail storm in Paris which did great harm to the conservatories, trees and plants of the *Jardin des Plantes*. M. Milne-Edwards has reported to the Paris Academy that it will be many years before the damage can be repaired.

ACCORDING to the *Botanical Gazette*, Dr. V. F. Brotherus, of Helsingfors, left about the middle of April upon a botanical journey in central Asia. He will explore the high mountain flora of Issikul, giving particular attention to the mosses.

THE London *Times* states that Captain H. H. P. Deasy has left England for a journey across Tibet from west to east. He intends on the way to throw away soldered-up tins containing parchment notices in English and French into the tributaries of the Tsanpo and into the other large rivers which he may meet with, in the hope that some of them may be picked up far down stream, possibly in the Brahmaputra, Salween, and Mekong, and thus help to solve the vexed problem of the origin and connections of these rivers. The notices will be consecutively numbered and the tins in which they will be enclosed will have a brass label soldered on the outside, bearing the words, 'Please open this' in English and French, and Captain Deasy's name. The parchment inside bears the request that it be forwarded without delay to the Royal Geographical Society, London, with as accurate a statement as possible as to where it was picked up.

ON the occasion of Prof. Bastian's seventieth birthday, which occurred on the 28th of June, his bust in marble was given to the Museum of Ethnology in Berlin. A *Festschrift* containing papers by 32 leading German anthropologists was also presented to him. Prof. Bastin is, as we have recently stated, now absent on an expedition through Central Asia.

PROF. VICTOR HORSLEY has been presented, on the occasion of his retirement from the chair of pathology, University College, London, with a testimonial including an album which contains photographs of fifty-one of the subscri-

bers to the testimonial, together with a record of the work which each of them has done either in conjunction with Prof. Horsley or in the Brown Institute and in the Pathological Department of University College during the time these laboratories were under his direction.

DR. L. H. BAUER, editor of *Terrestrial Magnetism*, is making a magnetic survey of Maryland under the auspices of the State Geological Survey, now being conducted by Prof. Clark, State Geologist.

GEORGE WHITEFIELD SAMSON, D. D., LL. D., formerly President of Columbian University, Washington, died in New York city on August 8th, aged 77 years.

THE deaths are announced of Prof. August Kanitz, Director of the Botanical Gardens at Klausenburg, and of Prof. F. R. Simony, the Alpine explorer, who died on July 20th, at the age of 83.

THE 'Leopolinisch-Carolinische Academie,' of Halle, is about to publish Cuvier's first composition, which is on the edible crabs of the French coast, and dates from the year 1788. The Academy has in its possession a number of letters of Cuvier, which it is also intended to publish.

THE Observatory of the School of Technology at Karlsruhe will be removed to Heidelberg. Its director, Prof. Valentiner, has been made professor in the University of Heidelberg.

AN observatory for terrestrial magnetism has been established in connection with the astronomical observatory in Munich and Dr. Franz von Schwarz has been made director.

THE Vienna Academy of Science proposes as the subject of the Baumgartner Prize ($5000), to be awarded in 1899, 'The extension of our knowledge of ultra violet rays.'

WE learn from *Natural Science* that a new zoological garden has been opened at Königsberg, in Prussia, under the directorship of Dr. J. Müller, formerly of the Garden in Berlin; also that a State Entomological Experiment Station, for which the money has been voted by both Chambers, is to be built near Stockholm.

THE first number of the twenty-second volume of the *Botanical Gazette* is published from the press of the University of Chicago, and, like the other journals of the University, is admirably printed on fine paper with wide margins. Profs. George F. Atkinson, of Cornell University, Volney H. Spalding, of the University of Michigan, Roland Thaxter, of Harvard, and William Trelease, of the Missouri Botanical Garden, will hereafter act as associate editors.

PROF. W. NERNST, of the University of Göttingen, has become the scientific editor of the *Zeitschrift fur Elektrochemie.*

GAUTHIER-VILLARS ET FILS announce an atlas containing fifteen large plates from photographs taken with the Röntgen rays by J. M. Eder and E. Valenta. The work on the same subject by E. E. Guillaume, also published by Gauthier-Villars, has reached a second edition.

IT appears that a new and very objectionable manner of advertising has been devised in Great Britain. In the bedrooms of some of the leading hotels, not only toilet articles, but also patent medicines are placed, in the hopes that guests will use and pay for these.

THE Paris Municipal Council authorized on July 10th the establishment of stands for automobile cabs. It is also considering the use of auto-mobile omnibuses, which, if adopted, will throw 27,000 horses out of employment! France seems to be distinctly in advance of England, Germany or America in the use of horseless carriages.

THE volume of *Biological Lectures* of the Marine Biological Laboratory, of Woods Holl, for 1895, is announced by Ginn & Co. as nearly ready. The volume will contain the following lectures: 'Infection and Intoxication,' Simon Flexner; 'Immunity,' George M. Sternberg; 'A Student's Reminiscences of Huxley,' Henry Fairfield Osborn; 'Paleontology as a Morphological Discipline,' W. B. Scott; 'Explanations or How Phenomena are Interpreted,' A. E. Dolbear; 'Known Relations between Mind and Matter,' A. E. Dolbear; 'On the Physical Basis of Animal Phosphorescence,' S. Watasé; 'Segmentation of the Vertebrate Head,' William A.

Locy; 'Segmentation of the Head,' J. S. Kingsley; 'Bibliography: A Study of Resources,' Charles Sedgwick Minot; 'The Transformation of Sporophyllary to Vegetative Organs,' George F. Atkinson.

WE learn from *Nature* that the observatory at Athens will hereafter publish a daily weather report containing twenty-five stations in Greece, and about double that number of exterior stations. The report is accompanied by two charts, one showing the isobars and general meteorological conditions over a large part of Europe, and one showing wind and temperature over Greece and adjacent islands.

THE New York *Medical Record* states that Rabies has broken out among the cows, dogs, sheep and hogs in Path Valley, Centre County, Pa., and it is feared that the disease will become general throughout the county.

ACCORDING to *Natural Science* a party of four, under the direction of Mr. T. A. Mobley, will start from Lacomb, Alberta, to explore northern Canada from Edmonton to the Arctic sea. The trip is to occupy two years.

ACCORDING to *Nature* the Russian Geographical Society has awarded this year its Constantine medal to M. A. Rykacheff, for his work in the domain of physical geography. Beginning in the year 1874 with a work on the distribution of atmospheric pressure in Russia, he continued to publish a series of researches on the diurnal variations of pressure, the prevailing winds of the Caspian and the White Seas, the tides in the atmosphere, the freezing and thawing of the Russian rivers, the variations of the levels of rivers in Middle Russia, in connection with variations in the amounts of rain and snow, the diurnal variations of temperature over the tropical oceans, etc. The Count Lütke medal has been awarded to Admiral Makaroff, for his work on the temperature and density of water in the northern Pacific, based on the measurements made in 1886–89 on board the *Vityaz*. His maps of the distribution of surface temperature in August, and of temperature at a depth of 400 meters, are especially worthy of notice. The Prjevalsky prize was awarded to M. Berezovsky, for his explorations of the northern borderlands of Tibet. A Prjevalsky medal was awarded to J. A. Schmidt, for his twelve geodetical expeditions to different parts of Central Asia and Siberia; and one to Dr. H. A. Fritsche, for his magnetic measurements in China, Mongolia, Siberia and Russia. Two small gold medals were awarded to F. F. Müller, for his magnetic work in East Siberia, and to A. A. Lebedintseff, for his researches into the chemical composition of water in the Black and Azov Seas. Eighteen silver medals were awarded for various works of lesser importance.

A PART of the collection of butterflies of the late Prof. A. M. Butljero has been presented to the Academy of Sciences of the University of Moscow. The collection of Mexican insects made by the late Julius Flohr has been bequeathed by him to the Museum of Natural History, Berlin; the same institution will also receive in event of the death of Herr Hans Fruhstorcher his valuable collection of butterflies.

ACCORDING to the London *Times*, some important alterations are now being carried out at Kew Gardens. The temperate house projected by the late Prince Consort was originally designed to comprise a central structure, two octagons and two wings. The wings were not added; but the government has now granted the necessary funds, and already such progress has been made that the south wing is nearly completed, and the north wing will be constructed at the earliest possible date. When finished, the temperate house will be one of the most striking, and probably the largest of the kind in existence. Its central avenue will be 600 ft. long, and there will be a clear view from end to end. This, it may be added, will be just double the length of the present largest house —the palm house—in the gardens. The building will be used especially for the accommodation of succulent plants, agaves, the taller cactuses, and the like, from such sub-tropical countries as the Cape, the highlands of Mexico and the Canary Islands. Limited in the area of their growth, the plants have hitherto been necessarily confined in tubs, but in their new quarters they will be planted out in beds, where they may be expected to flower and add a new

attraction to the Royal Gardens. There has just been established near what is known as the rhododendron dell in the arboretum a small bamboo garden. The species are mostly Japanese, Chinese and Himalayan and are probably the finest collection now in Great Britain. The naturalized bamboos have already created a great deal of interest, which may be followed by their more general cultivation. Two interesting official publications may also be mentioned—one in course of being issued and the other projected by the staff at Kew. The former is a complete list of the plants cultivated in the gardens, which it is hoped will fix a standard nomenclature, thus doing away with the confusion of identical plants known under two or more names. The other book will be a guide to the economic plants, and will include a summary of their qualities and uses.

THE London *Times* states that a Pasteur filter on a large scale has been exhibited by Messrs. J. Defries and Sons on their premises in Houndsditch. It has been constructed by them for the municipal authorities of Darjiling, with the sanction of the Indian government, and will be sent out there immediately. The filter consists of a series of iron 'cells,' or circular vessels, each of which contains 250 Pasteur porcelain tubes. These tubes resemble hollow candles and stand upright in the floor of the cell. The water filters through them from the outside to the inside under pressure or by gravity, and all impurities are arrested on the surface of the porcelain, which is formed of a special clay or mixture of clays. The filter is cleaned periodically, or as often as may be necessary, by scraping off the deposit from the surface of the tubes and by passing through them dilute hydrochloric acid, which dissolves and carries away the earthy salts deposited in the interstices of the porcelain. The principal of this filter and its successful application to domestic purposes have been well known for several years and have gradually overcome the distrust with which sanitary science is inclined to regard all fillters. Its efficiency seems to be well established not only by laboratory experiments, but—far more convincingly—by the practical results in the reduction of water-borne disease obtained by its use in the French army. The interest of the present exhibit lies in the size of the installation. It consists of 38 cells and 9,500 tubes, which will deliver 150,000 gallons per day.

As we are going to press with the present number of SCIENCE we have received provisional programs of the several sections of the approaching meeting of the American Association for the advancement of science. The number of papers entered at a date considerably in advance of the meeting is as follows: Section A. Mathematics and Astronomy, 8; Section B. Physics, 19; Section C. Chemistry, 60; Section D. Mechanical Science and Engineering, 23; Section E. Geology and Geography, 17; Section F. Zoology, 17; Section G. Botany, 35; Section I. Social and Economic Science, 9. In each of the sections other papers will doubtless be offered, which will be entered on the daily programs published during the meeting. It is a great advantage to have the programs in advance of the meeting and we regret that it is now too late to print them in full in this issue of SCIENCE. We hope to publish full reports of the sectional meetings, but no one who is able to be present at Buffalo should neglect to attend a meeting which promises to be of especial interest.

UNIVERSITY AND EDUCATIONAL NEWS.

THE HULL BIOLOGICAL LABORATORIES.

THE Chicago *University Record* gives an account of the exercises held on the occasion of the laying of the corner stone of the Hull Biological Laboratories on July 3d. The address in the convocation tent was given by Prof. G. L. Goodale, of Harvard University, who spoke on 'Some of the Relations of Natural History to Thought and Modern Life.' President Harper made a statement regarding the importance of Miss Culver's gift for the development of science, in the course of which he made the following important announcement regarding the place of investigation in the medical school: "In laying these corner stones to-day we are laying the foundations of a school of medicine, for aside from the distinct work outlined in each department there is that great and important

service to be rendered in the establishment of a school of medicine, the chief work of which shall be investigation."

The company then adjourned to the site chosen for the Laboratories, where the corner stones of the buildings were successively laid with appropriate ceremonial and addresses. Head Professor John M. Coulter gave the address at the foundation of the Botanical Laboratory; Associate Professor Jacques Loeb, at the site of the Physiological Laboratory; the address written by Head Professor Henry H. Donaldson was read by Assistant Professor E. O. Jordan at the site of the Anatomical Laboratory, and Head Professor Charles O. Whitman spoke at the Zoological foundation.

In the evening the University gave a dinner to Miss Helen Culver and the men of science present from other universities. Short speeches were made by Profs. Goodale, Barnes, Forbes, Burrill, McMurrich, MacBride and Holmes representing their respective institutions. Profs. Whitman, Loeb, Jordan and Coulter spoke in behalf of the biological faculties and Profs. Chamberlin and Judson on behalf of other departments. Finally President Harper told very simple the story of the gift, of its unexpectedness, of its coming entirely unsolicited and the manner in which it relieved the pressing wants and satisfied the most sanguine hopes of the departments concerned. At last late in the evening he announced that Miss Culver would say a few words. Then with the guests standing in their places at the tables, Miss Culver expressed very quietly her pleasure and satisfaction in being able to do what she could for the cause of higher education, and modestly claimed for herself only the credit of being an agent in carrying out what she felt would have been the desires of the man whose name the laboratories are to bear, Mr. C. J. Hull.

SCIENCE AT OXFORD.

WE called attention in a recent number of this JOURNAL to an important article in *Nature* on the position of science at Oxford. Prof. E. Ray Lankester, Linacre professor of zoology at Oxford, has addressed the following letter on the subject to *Nature:*

"Will you allow me a few lines in which to express my entire agreement with your recent article on this subject, if only to emphasize the fact that I am not the author of the article, and that the opinions there expressed are not those of an isolated individual. The reason for the comparative neglect of natural science at Oxford is that, however well-disposed some individuals may be, the college tutors and lecturers as a rule dislike it. They dislike it for two reasons: First, because it cannot be taught in the college parlors called lecture rooms; and second, because they are, as a rule, ignorant—owing to their own defective education—of the nature and scope of the immense field of study comprised under the head 'natural science.' They do not know either the enormous educational value of natural science, or its vital importance to our national life and development.

"And lastly, if they did know, there is no conceivable motive which could operate so as to induce them to sacrifice some of the rewards and educational domination, which are at present enjoyed by the long-established classical and historical studies, to newer lines of work in which the present beneficiaries and their academic offspring can have no share.

"The situation is a 'dead-lock,' and only an intelligent Parliamentary Commission (if such is possible) can put matters on to a fair and healthy basis. Probably the scandal of the present paralysis of our beloved Oxford will have to become even greater and more outrageous than it is at this moment, before the necessary remedy is applied.

"But happily the vitality of Oxford is indestructible. The misused and monopolized resources of Oxford will assuredly some day be devoted to the true purposes of a great university."

GENERAL.

THE authorities of Princeton University have issued a circular of information regarding the sesqui-centennial celebration which takes place on October 20th, 21st and 22d. The most important ceremonies will be held on October 22d, when degrees will be conferred and announcements will be made of the endowments secured. During the week preceding these exercises lectures will be given by some of the foreign

visitors. Prof. William Libbey is Secretary of the Reception Committee and should be addressed by those desiring programs or tickets.

WE learn from the *Academische Rundschau* that the total number of students matriculated in the German universities during the present summer semester is 29,864. This is 1,000 more than last summer and the largest attendance ever recorded, surpassing by about 400 the largest previous attendance which was in the summer of 1889. Berlin leads with 4,649 students, followed by Munich, Leipzig, Tübingen, Heidelberg, Erlangen and Göttingen, at each of which there were over 1,000. The students are distributed among the faculties as follows: Protestant theology, 2,959; Catholic theology, 1,502; law, 8,077; medicine, 7,931; pharmacy and dentistry, 1,415; philosophy, philology and history, 3,607; mathematics and natural science, 3,020; agriculture, 1,353. The total number of foreigners, including Austria and Switzerland, was 2,189, of whom 513 were Russians, 450 Americans, 139 English and 56 French.

THE *Botanical Gazette* states that Prof. Thos. A. Williams, professor of botany in the Agricultural College of South Dakota, has been appointed assistant in the division of agrostology of the Department of Agriculture. Mr. F. S. Earle, of the Alabama Polytechnic Institute, has been promoted to the professorship of biology made vacant by the removal of Prof. Underwood to Columbia University.

PROF. G. B. MATHEWS has resigned the chair of mathematics in University College, North Wales.

THE chair of mental philosophy and logic established sometime since in the University of Cambridge has never been filled, owing to lack of endowment. £700 annually have now been appropriated for the chair, £200 of which is due to the generosity of Prof. Sidgwick, and it is expected that a professor will soon be appointed.

SIR WALTER GILBY has founded in the University of Cambridge a lectureship on the history and economics of agriculture, having guaranteed for this purpose £25 a year for twenty-one years.

MR. S. HENBEST CAPPER, of Edinburgh, has been appointed to the newly-founded Macdonald Chair of Agriculture in the McGill University, Montreal.

THE following appointments are announced in German universities: Dr. Kurt Rümker has been made full professor of agriculture in the University of Breslau; Dr. Fr. W. Küster, professor of physical chemistry in the University of Göttingen, and Dr. Wilhelm Sandmeyer, professor of physiology in the University of Marburg. Dr. Max Fischer, of Halle, has been made professor in the Agricultural Institute at Leipzig. Prof. Hofmeister, of Prague, has been called to the chair of of physiological chemistry in the University of Strasburg; General M. Rijkatschef has been appointed director of the Physical Observatory in St. Petersburg, as successor to Dr. Wild. Dr. Richard Lorenz of Göttingen, has been made professor of electro-chemistry in the Polytechnic Institute in Zurich. The railway inspector, Herr Troske, has been appointed professor of engineering in the Technical High School of Hanover. Dr. Schleiermacher, of the Technical High School in Karlsruhe, has been promoted to a full professorship of electro-chemistry, and Dr. Schuberg, of the University of Heidelberg, to an assistant professorship of zoology. Dr. J. Biehringer has been appointed docent in general and technical chemistry in the Technical High School in Braunschweig, and Dr. Benecke docent in botany in the University of Strasburg.

DISCUSSION AND CORRESPONDENCE.

GIFTS TO THE LICK OBSERVATORY.

MISS CAROLINE W. BRUCE, of New York City, has given the observatory a sum of money to procure a large comet-seeker, and to provide photometers for visual use with the thirty-six-inch equatorial.

Mr. Walter W. Law, of Scarboro'-on-Hudson, has likewise made a liberal gift towards providing for the publication of the Observatory Atlas of the Moon mentioned in the *Publications*, Volume VIII., page 187. The grateful thanks of the Observatory are offered to these friends, who have made it possible to undertake new work. EDWARD S. HOLDEN.

MOUNT HAMILTON.

ON LIFTING MONOLITHS.

DEAR SCIENCE: It is a subject upon which I have frequently thought, but concerning which I have seen nothing written, that all the megalithic and Cyclopean structures of the world were erected at a time or under circumstances that may be called pre-mechanical. Neither in America nor in any other part of the world has the account of the moving of a 'big stone' been written down. There is not a modern machine capable of lifting some of these great stones and herein lies the secret. If you will examine the twine, sennit, cables, ropes of modern savagery, you will at once see that in prehistoric times machinery could not have been utilized in lifting the great monoliths. There was not in all the world, during the periods when the megalithic monuments were being set up a derrick, or chain, or rope, capable of sustaining the weight. In Washington the stone cutters and contractors do not dream of hoisting the big stones that form the bases of monuments, though they are only pebbles compared with those of Teotihuacan or Baalbec. They move them on rollers, by means of crowbars and capstans turned by men or mules or horses, simple enough to have been familiar to the ancients. But even such affairs would be like rags hitched to a stone weighing a hundred tons or more. There is no use in looking for the machinery for the transportation of the megaliths; there was none. Time was the essential factor. A people that could pry up one end of a stone could put a roller under it. If they could move it twenty feet in a day, that would be over a mile in a year. Flotation, cribwork, inclined planes, levers, wedges were the utensils of horizontal and vertical motion. Count Wurmbrand has figured, in Matériaux pour l'histoire primitive et naturelle de l'homme, a company of men in India carrying a menhir upon a framework of wood and bamboo. If two hundred men could get around such a device and each bore two hundred pounds, the total weight could not exceed twenty tons. In studying the history of architecture one is almost justified in thinking that the size of the stone lifted has steadily decreased with the perfecting of lifting devices. Speed is the point aimed at. To fill a given space the modern crane derrick will do the work quicker with small blocks and much cheaper from every point of view than it could be done with a single large block. Without dwelling further upon the economic side, the fact remains that all the megalithic and cyclopean structures of the world were erected by means of the cooperation of human hands, using the simplest mechanical powers and without lifting machines of any kind.

OTIS T. MASON.

THE 'KANSAN' GLACIAL BORDER.

TO THE EDITOR OF SCIENCE: I have been extending the delimitation of the 'Kansan' glacial border westward from Lock Haven, Penna., during the past month, and a few of the points noted are of more than ordinary interest.

The first is regarding the possible existence of two glacial lobes from northeast and northwest which met and neutralized one another over the area north from Bradford, Penna., instead of proceeding south along the level valley of the Tunangeawant. A comparison of the 'Wisconsin' border of Lewis & Wright and the 'Kansan' border shows that they approach one another and almost coincide at the New York apex, while they diverge more and more as they extend southward. The 'Kansan' portion of the eastern lobe is lacking in fragments of crystalline rocks, while the same portion of the western lobe carries them. A study of the moraines of recession will easily settle the question thus proposed.

The second is that the 'Kansan' deposits over the Allegheny region bear out the deductions made from a study of similar deposits in eastern Pennsylvania that there has been but one epoch and that of comparative recency. A great deal of discussion has gone on regarding alleged 'high-level gravels' in the Allegheny region. This was on the basis of the 'Wisconsin' border being the extreme limit of ice action. The work of the past month shows that the Allegheny river was completely covered with ice as far south as Franklin (where the work is now being carried on), and all the localities noted by Messrs. Chamberlin, Wright and others along the tributaries to the Alle-

gheny from the north were covered by the Kansan advance and filled with its débris, They were afterwards more or less excavated and filled with later modified Wisconsin material; but immediately and at a moderate interval in the past, as is shown by two facts: the state of the crystallines in the Kansan drift, and the condition of the river gorges.

The writer, several years ago (*Am. Jour. Sci.*), made the statement that the majority of glacial students seemed to have failed to consider the state of the surface immediately before the first glacial advance. He, thereupon, stated that all portions of surface outcrops too hard to be ground into flour would form a rusty gravel, with the rustiness due to previous weathering, and not to lapse of time since deposition. This is fully proved in the Kansan drift in western Pennsylvania, where red granite cobbles have been found on top of the hills east of the Allegheny river, and from four to five hundred feet above it, and these have been glaciated on one or two sides, where the smoothed surface acquires the aspect of 'rusty gravel,' while on other sides the old surface weathering remains undisturbed to such an extent that the rock has lost entirely its black bisilicates, is completely kaolinized and is pulverulent. One side is scraped down to the hard and rusty interior, and the other remains as it lay on the surface when picked up by the ice. In the same way local Pocono and Carbonic sandstones will show a hard glaciated surface and a pulverulent angular surface in the same fragment and in hundreds of instances. These lie in red clay on local white sandstone. With these ancient relics are sparingly mixed river-rolled sandstones and shales as highly polished and as hard as any in the Wisconsin deposits. These are found under conditions which exclude their being residual from local weathered conglomerates, and, as in eastern Pennsylvania, they bear witness to the close association of Kansan and Wisconsin formations.

The best proof, however, lies in the state of the river bottoms. My assistant, Mr. Joseph Barrell, has discovered and studied the abandoned channel, not hitherto noted, of Oil Creek below Petroleum Centre, and will discuss it fully later. I wish to call attention to the importance of his discovery that both the old and the present channels are of equal depth; both are filled with Kansan and modified Wisconsin drift, and the creek has not cut down to its preglacial or Kansan level since the glacial epoch.

As the finding of Kansan drift over the region shows that high and low level gravels could accumulate from the same source, so the discovery of this filled valley, under exactly similar conditions which obtain in the Lehigh region, shows that Kansan and Wisconsin advances, as far as the State of Pennsylvania is concerned, were closely allied and not very remote.

EDWARD H. WILLIAMS, JR.
LEHIGH UNIVERSITY, August 3, 1896.

A LARGE LOBSTER.

THE subject of the size attained by the lobster has been recently treated by Herrick in his work on the Habits and Development of the American Lobster.

Various exaggerated reports of lobsters weighing 30 to 40 pounds have appeared as newspaper items, but the authenticity of such statements is questionable.

Herrick describes a specimen captured at Boothbay, Me., in 1891, and now in the museum of Adelbert College, which is probably the largest on record which has received accurate measurements.

On April 10, 1896, there was captured near Block Island a fine specimen which closely approaches in size the one described by Herrick. This was entangled in a trawl line in deep water, and so captured. It passed through the hands of Mr. E. C. Smith, a lobster dealer of Newport, R. I., and is now in the possession of Mr. F. W. Wamsley, of Woods Holl. It is destined for the museum of the Academy of Natural Science at Philadelphia.

The specimen is a male, perfect in every respect, and weighed alive slightly over 22 pounds. I have carefully measured it and find that the total length from tip of rostrum to end of telson is 21 inches. The greatest breadth of carapace is $5\frac{1}{2}$ inches, while the girth just behind the cervical groove, from edge of branchiostegite of one side to same position on other side, is $13\frac{5}{8}$ inches. The crushing chela is on the left side. The

length of its propodus is $13\frac{1}{4}$ inches; the girth just proximal to dactyl is $16\frac{1}{2}$ inches. The propodus of cutting claw is somewhat smaller; length $12\frac{3}{4}$ inches, girth $12\frac{1}{4}$ inches.

The pleon is 11 inches in length, and the girth of tergum of second segment—spine to spine—is $8\frac{5}{8}$ inches.

Apparently this specimen is larger than the one described by Herrick, if we consider only the length. This is due to the perfect rostral spine, which was broken in the Boothbay specimen. If we take the length from base of rostrum to tip of telson—a fair measurement to give an idea of bulk—we find the Block Island specimen is 19 inches, while the one from Boothbay is $19\frac{1}{4}$ inches.

By taking the average of the differences in measurements of the two specimens, I find that the one described by Herrick is larger by about six per cent.

While therefore this specimen is not the largest on record, its perfect condition warrants its description, as it so nearly approaches the maximum in size of the American lobster so far authentically reported. F. C. WAITE.

HARVARD UNIVERSITY, August 1, 1896.

SCIENTIFIC LITERATURE.

Mars. By PERCIVAL LOWELL. Boston: Houghton, Mifflin & Co. 1895. 8°. Pp. 228+viii; xxiv illustrations.

I am pleased to comply with the Editor's request for a review of Mr. Lowell's interesting book.

The reviewer of a work on organic evolution would find it difficult to avoid mentioning Darwin. Schiaparelli holds a similar place in the literature of Mars. An intelligent criticism of any recent book on Mars must consist largely of a review of Schiaparelli's observations and ideas. Of his predecessors it will be well to mention, for the benefit of non-astronomical readers, the following: (*a*) Galileo (1610), who discovered the phases of the planet, thereby proving that its light, though very red, is really reflected sunlight; (*b*) Huyghens (1659), who first observed marking on the surface; (*c*) Cassini (1666), who determined the length of the Martian day, and discovered the white polar caps; (*d*) Sir William Herschel (1783), who observed the waxing and waning of the polar caps with the seasons; (*e*) Beer and Maedler, who published the first map on the planet's surface features, and discovered at least three of the so-called canals; (*f*) Dawes (1864), whose drawings show a dozen of the canals; and (*g*) Hall (1877), who discovered the two satellites.

Schiaparelli's work extends continuously from 1877 on. It is impossible to do justice to his labors in this article. He extended our knowledge of the planet enormously in nearly every line—in reference to the polar caps, the so-called seas and continents, but especially in reference to the so-called canals, their appearance and disappearance, their doubling, etc. His entire work bears the impress of a scientific spirit *par excellence.* His observations cover the period 1877–92, but his technical results are comprised in a few papers, and a dozen 8vo. pages suffice for a masterly popular exposition of his general results. His brief papers contain at least the suggestion of all the theories recently exploited by popular writers, though he was not concerned with establishing a theory, but rather with ascertaining facts.

Schiaparelli's remarkable observations of the network of straight canals and their doubling were questioned for years, but the confirmation they finally received at Nice and elsewhere largely removed the doubt.

Mr. Lowell's book on Mars is based upon the Flagstaff, Arizona, observations made by himself between May 31 and November 20, 1894, and by Prof. W. H. Pickering and Mr. A. E. Douglass between May, 1894, and April, 1895. Mr. Lowell delivered a lecture under the auspices of the Boston Scientific Society, on May 22, 1894, in which he is reported (*Boston Commonwealth* for May 24, 1894,) to have announced that his observatory—not yet completed—was for the purpose of making "an investigation into the conditions of life in other worlds, including last, but not least their habitability by beings like or unlike man. This is not the chimerical search some may suppose. On the contrary, there is strong reason to believe that we are on the eve of pretty definite discovery in the matter."

Speaking of Schiaparelli's canals on Mars, Mr. Lowell is reported to have said in his lec-

ture, "the most self-evident explanation from the markings [canals] themselves is probably the true one; namely, that in them we are looking upon the result of the work of some sort of intelligent beings. * * * The amazing blue network on Mars hints that one planet besides our own is actually inhabited now. * * * We stand upon the threshold of a knowledge of our closest of kin in the world of space, of the the most important character."

Mr. Lowell went direct from the lecture hall to his observatory in Arizona, and how well his observations established his pre-observational views is told in his book. In outline his conclusion is that there is a scarcity of water on Mars; that the melting of the polar snows is the source of water supply for the planet; that a network of straight canals conducts the water from the poles over the planet; that what we see and call canals are not water, but vegetation along the banks—a suggestion made several years ago by Schiapparelli and by Prof. Pickering; that since the canals are all straight, *i. e.*, run on great circles, and are of uniform width, and in general several of them intersect in one point, then they probably are the handiwork of the Martian inhabitants; that the planet is probably inhabited by highly intelligent beings; and that the irrigation problem is their chief concern.

It will be seen that Mr. Lowell's results agree perfectly with his pre-observational views quoted above; but in justice to him it must be said that he has written vigorously and at length (pp. 158–161) of the dangers of bias on the part of those having preconceived notions, and in numerous paragraphs throughout the book severely criticises those who write on the subject without having made the observations. So I suppose we shall have to forget his remarkable preliminary lecture.

Before examining Mr. Lowell's evidences of intelligent beings on Mars, let us look at his idea of how the world would receive such a discovery. He believes the world would not welcome it. "To be shy of anything resembling himself is part and parcel of man's own individuality. * * * The civilized thinker instinctively turns from the thought of mind other than the one he knows." Various astounding hypotheses "commend themselves to man, if only by such means he may escape the admission of anything approaching his kind. * * * It is simply an instinct like any other, the projection of the instinct of self-preservation."

Here Mr. Lowell is certainly wrong. In my opinion, he has taken the popular side of the most popular scientific question afloat. The world at large is anxious for the discovery of intelligent life on Mars, and every advocate gets an instant and large audience. Scientific men are quite ready to admit the possibility of life wherever the environment is shown to be suitable. While we can safely say that other suns than ours have their planets and some of those planets probably support life, yet only two cases have come under satisfactory observations: the Earth and the Moon. The former is inhabited; we may safely say the latter is not. In size certainly, and in physical condition probably, Mars is somewhat nearer the Moon that the Earth; and while the affirmative side of the question, 'Is Mars inhabited?' will get at least a just hearing, those who advocate that side must prepare the burden of proof.

Speaking of the melting of the northern polar cap of Mars, Schiaparelli wrote in 1892: "From this arises a singular phenomenon which has no analogy upon the Earth. At the melting of the snows, accumulated at that pole during the long night of ten months or more, the liquid mass produced in that operation is diffused around the circumference of the snowy region, converting a large zone of surrounding land into a temporary sea and filling all the lower regions. This produces a gigantic inundation. * * * The white spot of snow is surrounded by a dark zone, which follows its perimeter in its progressive diminution, upon a circumference ever more and more narrow. The outer part of this zone branches out into dark lines, which occupy all the surrounding region, and seem to be distributary canals by which the liquid mass may return to its natural position. This produces in these regions very extensive lakes. * * * This inundation is spread out to a great distance by means of a network of canals, perhaps constituting the principal mechanism (if not the only one) by which water

(and with it organic life) may be diffused over the arid surface of the planet; because on Mars it rains very rarely, and perhaps even it does not rain at all. * * * Such a state of things does not cease until the snow, reduced to a minimum area, ceases to melt. Then the breadth of the canals diminishes, the temporary sea disappears, and the yellow region again returns to its former condition. The different phases of these vast phenomena are renewed at each return of the seasons, and we have been able to observe them in all their particulars very easily during the oppositions of 1882, 1884 and 1886, when the planet presented its northern pole to terrestrial spectators. The most natural and most simple interpretation is that to which we have referred, of a great inundation produced by the melting of the snows. * * * We conclude, therefore, that the canals are such in fact, and not only in name. * * * that the lines called canals are truly great furrows or depressions in the surface of the planet, destined for the passage of the liquid mass and constituting for it a true hydrographic system."*

At the 1894 opposition the axis of Mars was tilted so that the region between the south pole and 40° north latitude was presented to terrestrial observers, the north polar region being hidden from sight. Mr. Lowell's observations covered one-fourth of the Martian year, from May 1st to August 1st, Martian time. His book pays special attention to the melting of the south polar cap, and to what he considers to be the train of related phenomena; since around and upon those phenomena he builds his argument for intelligent life on that planet. On May 1st, Martian time, the south cap was "in rapid process of melting. * * * As it melted, a dark band appeared surrounding it on all sides. Except, as I have since learned, at Arequipa, this band has never, I believe, been distinctively noted or commented on before, which is singular, considering how conspicuous it was at Flagstaff." (This last sentence is indeed surprising, as scores of drawings published in 1892 and earlier show this dark band very conspicuously; it is well known to all observers of Mars, and Schiaparelli's description of the same phenomena at the melting of the north polar cap is very familiar.) "As the snows dwindled, the blue band shrunk in width to correspond," and finally, when the cap had entirely disappeared, its encircling dark band had also vanished. Mr. Lowell believes the dark band was water, and that it disappeared by flowing away from the pole towards the equator in canals, circulating through the planet's arid regions. In proof thereof he submits that he has observed a slow wave of dark area to advance equator-ward from the poles; that the canals nearest the south pole grew dark and thereby became visible first; then those nearer the equator; then those at the equator; and finally those north of the equator; in other words, in the order that water flowing from the south pole would reach different parts of the planet.

It will be seen that the Flagstaff observations upon the melting of the south polar cap and the flow of water therefrom are identical with those made (and published) by Schiaparelli in the case of the north polar cap in 1882, 1884 and 1886; but these observations by Schiaparelli are not mentioned in Mr. Lowell's book. The Flagstaff observations in a measure confirm Schiaparelli's general results and extend them to the region of the south pole.

Of the origin of the canal system Schiaparelli writes entertainingly: "Their singular aspect, and their being drawn with absolute geometrical precision, as if they were the work of rule or compass, has led some to see in them the work of intelligent beings, inhabitants of the planet. I am very careful not to combat this theory, which includes nothing impossible. * * * The network formed by these was probably determined in its origin in the geological state of the planet, and has come to be slowly elaborated in the course of centuries. It is not necessary to suppose them the work of intelligent beings; and notwithstanding the almost geometrical appearance of all of their system, we are now inclined to believe them to be produced by the evolution of the planet, just as on the Earth we have the English Channel and the Channel of Mozambique."

Of the gemination of the canals Schiaparelli

* For this and other passages from Schiaparelli's Italian papers I am indebted to Professor Pickering's translation in *Astronomy and Astro-Physics*, 1894.

writes: "In consequence of a rapid process, which certainly lasts at most a few days, or even perhaps only a few hours, * * * a given canal changes its appearance, and is found transformed through all its length into two lines or uniform stripes, more or less parallel to one another, and which run straight and equal with the exact geometrical precision of the rails of a railroad. * * * One of these is often superposed as exactly as possible upon the former line, the other being drawn anew. * * * But it also happens that both the lines may occupy opposite sides of the former canal and be located upon entirely new ground. The distance between the two lines differs in different geminations, and varies from 360 miles and more, down to the smallest limit at which two lines may appear separated in large visual telescopes—less than an interval of 30 miles." Schiaparelli explains that the variations might be the result of "extensive agricultural labor and irrigation upon a large scale. Let us add further that the intervention of intelligent beings might explain the geometrical appearance of the gemination, but it is not at all necessary for such a purpose. The geometry of nature is manifested in many other facts, from which are excluded the idea of any artificial labor whatever. * * * It would be far more easy if we were willing to introduce the forces pertaining to organic nature. Here the field of plausible supposition is immense, being capable of making an infinite number of combinations, capable of satisfying the appearances even with the smallest and simplest means. Changes of vegetation over a vast area * * * may well be rendered visible at such a distance. * * * For us, who know so little of the physical state of Mars and nothing of its organic life, the great liberty of possible supposition renders arbitrary all explanations of this sort, and constitutes the gravest obstacle to the acquisition of well founded notions."

Such, in effect, is all that Schiaparelli has written by way of explanation of his remarkable discoveries, and he who runs may read his scientific mind.

Mr. Lowell's book contains a beautiful map of the portion of Mars lying between 70° south and 40° north latitude (on Mercator's projection). It represents the *ensemble* of the individual sketches made by Messrs. Lowell, Pickering and Douglass at Flagstaff in November, 1894. It contains 183 canals, lying both in the light and dark regions of the planet. Of those lying in the light reddish regions, 63 appear to be identical with those discovered by Schiaparelli and his predecessors, and 72 appear to be new. Mr. Douglass is credited with the discovery of 44 canals in the dark regions of the planet. I infer from Mr. Lowell's book that the canals in the dark regions were not seen and confirmed by either Mr. Lowell or Prof. Pickering, though they were observing Mars at the same time and place. Evidently, then, these observations at Flagstaff were difficult, and Mr. Lowell considers them to be new, though they are not new. In 1892 Prof. Schaeberle observed them, and wrote that "Crossing the darker areas are still darker streaks which often extend hundreds of miles in nearly straight lines. One end of a given streak usually terminates in the equatorial region at a point where the dark area protrudes into the bright area, and the so-called canals seem to be continuations of the streaks" (*Publications* Ast. Soc. Pacific., iv., 197). It was often noticed in 1894 by the writer and other Lick observers that the dark areas on Mars were composed of a mass of details so complex as to defy the draughtsman's skill; but I think Mr. Douglass, at Flagstaff, is the only observer who has verified Prof. Schaeberle's 1892 observations that these markings were arranged in nearly straight lines. If the observations by Messrs. Schaeberle and Douglass are to extend the canal system over the dark areas, just as Schiaparelli's extend them over the bright areas, they constitute a most important advance in Martian work. The recent observations of canals or other details within the dark areas, the recent spectroscopic and polariscopic observations, all strongly oppose the favorite theory that the dark areas are seas, but support the common theory that the bright areas are land.

Mr. Lowell observed a few double canals, probably a fourth as many as Schiaparelli saw.

At the exact point where two or more canals cross each other the observers noticed that

there was in nearly every case a dark circular or oval spot acting as the hub from which the canals radiated as spokes. To these swollen junctions Mr. Lowell applies the name 'oases.' A few of these spots were observed by Schiaparelli and others, but the Flagstaff observers have greatly extended the list.

As explained above, Mr. Lowell accepts the suggestion made by Schiaparelli and others that the canals form the planet's hydrographic system; that the changes observed may be due to vegetation, to irrigation on a large scale. He holds that the *visible* canals and the 'oases' are due to vegetation along the lines of the *real* canals; and that the whole system essentially proves, or at least renders it very probable, that Mars is inhabited by a highly intelligent race whose chief concern is irrigation. His argument is made with great skill. Every fact is considered to point in that direction, and every observed phenomenon is considered to be accounted for, though in explaining the mysterious doubling of the canals he admits that "we are here very much in the dark." It is held that the canals being vegetal in character, and watered from the melting snow at the poles, are seasonal, developing in the order of their distance (in time) from the poles, and reach their highest development at or shortly after the time of summer solstice. Such, in fact, is the train of phenomena which Mr. Lowell claims to have observed, starting from the south pole and extending to about 40° north latitude. Schiaparelli observed similar phenomena in the vicinity of the north pole, when that region was in position for observation. His sketches made at or shortly after the northern summer solstice cover the region from the north pole to about 40° south latitude.

Let us examine Mr. Lowell's irrigation scheme. A hydraulic engineer would ask some questions which Mr. Lowell does not discuss in his book. In the southern summer Mr. Lowell has the planet's surface covered with canals running in every direction, from the south pole to at least 43° north latitude; as far as the tilted position of Mars permitted him to see. We do not know but that they extended entirely to the north pole. In the northern summer Schiaparelli's system of canals extended from the north pole southward to 30° south latitude, or further; in fact, as far as the position of the planet permitted him to see. And it is agreed by Mr. Lowell that his principal canals are identical with Schiaparelli's. So we are asked to believe that the equatorial region of Mars, forming a strip at least 70° wide, can be and is irrigated from both the north and south poles; the 'canals' in the two cases of opposite flow being identical! The corresponding problem on the Earth would be to irrigate San Francisco, Chicago, New York, Rome, Tokyo, from the snow melting at the South Pole; and to irrigate Valparaiso, Cape of Good Hope, Australia, from the snow melting at our North Pole: all the irrigated land lying between New York, etc., on the north and the Cape of Good Hope, etc., on the south to be irrigated alike from the North and South Poles. Mr. Lowell ventures no explanation of how this engineering problem is to be worked out, though he states that the canals form a system "precisely counterparting what a system of irrigation would look like; and, lastly, that there is a set of spots placed where we should expect to find the land thus artificially fertilized, and behaving as such constructed 'oases' should."

If the visible canals are due to irrigated vegetation in strips 30 to 60 and more miles wide, traversing the planet's surface in straight lines in every direction, all the canals hundreds and many of them thousands of miles long, from four to ten canals radiating from a common point, intersecting at all angles a great many other canals radiating from other centers, how is the water distributed over this large and complex area? It starts from the polar snows, we are told, and flows thousands of miles to and beyond the torrid zone, spreading in a general way over the whole planet. Do these streams lie in the valleys, or on the slopes and ridges? There is no evidence whatever that the surface is remarkably level. The canals, apparently, do not turn aside for anything. The path of least resistance seems to be unknown.

The crater *Tycho*, on our moon, is the center of a system of markings radiating in all directions in straight lines, hundreds and thousands of miles. They cross hills and valleys with per-

fect indifference. Because they are straight and radiate from a center, did they have an intelligent personal origin?

Is a seasonal change on Mars evidence of an intelligent population? The virgin forests and prairies of America donned and doffed their annual green suit even better before the advent of man than to-day.

The organic origin of the dark areas on Mars has great advantages, as Schiaparelli said; but the addition of intelligent beings to the hypothesis adds to, rather than removes, the difficulties, and leads to pure speculation. If we attempt an explanation of the irrigation system we can, in our dilemma, only say that the Martians are more intelligent than we are!

The most striking feature of the Flagstaff observations relates to the detection of a large number of canals and 'oases.' It is a question how far these observations have had confirmation, and how far they need it. The observation of 44 canals in the dark areas by Mr. Douglass confirms Prof. Schaeberle's 1892 observations, but they were evidently not seen by Messrs. Lowell and Pickering. Mr. Lowell gives a long list of canals in the bright areas, but it is uncertain whether or not they were seen by more than one observer. His list contains nine canals that were seen on only one occasion; they are drawn on the final map and given names. His list contains one canal that *was not seen at all*, but on *one* occasion was *suspected*; it is put on the map and given a name.

Mr. Lowell accepts the line of reasoning put forth by Proctor and others as to the extent of Mars' atmosphere, viz.: That the mass of terrestrial atmosphere is to the mass of Mars' atmosphere as the mass of the Earth is to the mass of Mars; which leads to the result that the density of the atmosphere at the surface of Mars is about half the density of our atmosphere at the summit of the Himalayas. This is in complete harmony with the Lick spectroscopic results of 1894, which pointed to that density as the maximum limit, but is quite out of harmony with the earlier spectroscopic results.

It is well known that the atmosphere of Mars is practically cloudless. There is some evidence of clouds near the terminator (sunrise and sunset line), and some in favor of occasional small clouds over the portions fully exposed to the sun's light and heat. For two or three weeks in October, 1894, all the surface features were partially obscured and rendered indistinct, as if by general haziness, after which they again became distinct. Mr. Lowell believes that the Flagstaff observers saw several hundred clouds near the terminator, though he makes no use of them in explaining Mars' hydrographic system. They are not needed for irrigation purposes. The atmosphere is supposed to be very rarefied, the polar snows melt, the water in some manner evaporates into the atmosphere to form the polar caps by precipitation the following winter. If snow is precipitated at the cold poles, why should not rain be precipitated in the warmer regions? If the atmosphere is thin and takes up the evaporated water in a clear noon sky, why should not the rarefied atmosphere cool rapidly at night and rain be precipitated, especially in the valleys? If the atmospheric circulation is slow, as it is supposed to be, the visible effects of night rains could well progress from the poles toward the equator, through the valleys, and a delicate system of surface levels would not have to be provided. This is not put forth as a theory of the canal system, except to emphasize the fact that we should give Nature a chance to do this work before we resort to artificial irrigation.

In 1890 there began at Mount Hamilton a new class of observations on Mars, relating to the bright projections on the terminator. Similar observations were made in 1892 and 1894. There is no doubt that they are very important, and great stress was laid on them. There are some arguments in favor of there being clouds, but many more in favor of there being mountains. The observed phenomena are fully explained by supposing a mountain chain to lie across the terminator and to disappear from sight by the planet's diurnal rotation. The observed projections were such as would be produced by the sun shining on the mountain tops outside the terminator, and the observed adjacent depressions were such as would be formed by the shadow of the mountain range lying within the terminator. Concerning the

1894 Flagstaff observations of the terminator by Mr. Douglass, Mr. Lowell writes that "Of the 736 irregularities observed, 694 were not only recorded, but measured. Of these, 403 were depressions. It is singular, in view of their easy visibility, that, with the exception of Schroeter, in the last century, no one should have noticed them before."

Mr. Lowell rejects 346 out of 403 depressions as not real, since they lay on the dark areas of the planet and were due to the smaller irradiation at those places. He holds that the remaining 57 depressions were due to clouds within the terminator, and 291 projections were clouds outside the terminator; because if they were mountains the number of depressions should equal the number of projections. To my mind, the argument is not convincing. If we remove 196 of the projections which are described as 'long and low,' and which some experience in observing them leads me to ascribe to excessive irradiation, we shall have 95 projections and 57 depressions of the 'short and sharp variety.' When we consider that these clouds or mountains (or something else) are immersed in an illuminated atmosphere, we cannot expect the projections and depressions to be equal in number. The problem will not be settled until it is determined whether or not the projections occupy fixed and the same positions at many successive oppositions—the phase and atmospheric conditions being equal.

I confess my inability to unravel Mr. Lowell's discussions of Mr. Douglass' observations. When it was a question of detecting a twilight effect it was the illuminated atmosphere which formed the visible and measurable terminator. When it was a question of proving that Mars was extremely level, and would, therefore, lend itself to general irrigation, it was the land surface that formed the visible terminator; and since this terminator was always "comparatively smooth, * * * we know that, relatively to his size, he has no elevations or depressions on his surface comparable to the lunar peaks and craters." Lastly, the several hundred irregularities observed on the terminator, varying from those extremely high to those very low, were attributed to clouds. The terminator, then, is formed by the illuminated atmosphere and not by the land surface; secondly, there are no significant elevations and depressions on the surface, because the terminator, formed by the land surface, is comparatively smooth; and thirdly, the extensive irregularities on the terminator, which 'may be seen every night,' are due to clouds.

Mr. Lowell writes of the 'long and low' irregularities that the projections averaged $0''.136$ in height; the depressions $0''.125$ in depth. These are the distances from the approximately elliptic arc that would have formed the apparent terminator if the irregularities had not existed. Thus we have the heights of the irregularities from a curve that did not exist given to three decimals of a second of arc! And there is nothing to show that the varying distances of the planet were taken into account, either. Every practical astronomer knows that the *first* decimal place is uncertain; the systematic errors in such cases can easily and generally do exceed a tenth of a second. To say that the results are accurate because they are the mean of a large number of observations is to say that if a stranger to Colorado's clear atmosphere should waken unexpectedly on Pike's Peak and guess the distances to several hundred neighboring peaks, the mean of all the guesses would be very near their average distance.

There is not much demand for mathematical analysis in a popular book on Mars, nor is the application of that little always happy. On pages 133–134, after stating that practically all the canals follow the arcs of great circles, and necessarily appear curved when viewed obliquely, the author writes, "apparent straightness throughout is only possible in comparatively short lines. For a very long arc [of a great circle] upon the surface of a revolving globe tilted toward the observer to appear straight in its entirety it must lie due north and south." This is incorrect. If the apparent center of the planet's disc is at 18° south latitude, which was the average for Mars in 1894, then every arc of every great circle that can be drawn in any direction through any point that lies on the minus 18° circle of latitude will appear straight twice every day. An infinite number of such circles can be drawn. Mr.

Lowell's misconception of the mathematical principles of the 'great circle' is fundamental. Does it render null and void his conclusion that the canals lie on arcs of great circles?

Mr. Lowell found that the surface markings on Mars came to the central meridian about twenty minutes later than the predicted time; a discrepancy, it should be said, to which Prof. Keeler called special attention in 1892.

To what extent Mr. Lowell's future observations will modify his map is uncertain. Drawings of Mars by different observers even on the same night and with the same telescope are proverbially different. So far as the drawings by the three Flagstaff observers have been published, the proverb still seems to be in force.

Mr. Lowell is entitled to great credit for devoting his private means so generously to establishing and conducting an observatory, and for his efforts in search of the best, but imperfect, atmospheric conditions. He is likewise fully aware of the necessity of making the observations continuously and systematically. Whatever advances Mr. Lowell may have made in Martian study, or may make in the future, will be fully accredited to him and warmly welcomed by all astronomers.

Mr. Lowell's book is written in a lively and entertaining style, and is printed and illustrated faultlessly. It is true that the theories advanced are mostly old ones, suggested by Schiaparelli, Pickering and others, many of them having been elaborated by Flammarion and others; but Mr. Lowell has presented them very fully and suggestively. Scientifically, the leading faults of the book are: First, that so elaborate an argument for intelligent life on the planet, embracing a complex system of seasonal changes, should be based upon observations covering only one-fourth of only one Martian year; and, secondly, that there should be so many evidences of apparent lack of familiarity with the literature of the subject.

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Text-book of the Embryology of Invertebrates. By DR. E. KORSCHELT and DR. K. HEIDER. Translated from the German by EDWARD L. MARK, PH. D., and W. MC. WOODWORTH, PH. D., with additions by the authors and translators. Part I.: Porifera, Cnidaria, Ctenophora, Vermes, Enteropneusta, Echinodermata. New York: Macmillan & Co. 8vo. Pp. xv+484. 1895. $4.00.

The first Heft of the special part of Korschelt and Heider's well-known Lehrbuch, of which this is the English translation, appeared in 1890; the second Heft appeared in 1892 and the third in 1893. The three parts together form a volume of some fifteen hundred pages, illustrated by some nine hundred figures. They complete the special part of the work, that which presents the facts of embryology. A general part, to deal with theories and conclusions, is promised.

The first volume of Balfour's Comparative Embryology, dealing with invertebrates, appeared in 1890, and following that, the work of Korschelt and Heider was the first attempt at a 'broad and comprehensive' treatment of the whole field of invertebrate embryology. The book has been for several years in the hands of zoologists all over the world and is recognized as an excellent and indispensable reference book, the only one of its kind since Balfour. The labor involved in reading the special papers dealing with each group of animals treated and in sifting and arranging their results is so enormous, and the work of Korschelt and Heider has been so well done, that the book is likely for many years to remain without a rival. It is too well known to need critical treatment in this place.

The translation under consideration covers the first three hundred and twenty pages of the original. The remaining four-fifths of the book is to be rendered by another translator.

The German has been more freely rendered than in Mark's translation of Hertwig's Textbook of Embryology, and this gives the present book better literary form and makes it easier reading. At the same time the original has been so closely followed that nothing is lost or its meaning. The few instances where the English is not perfectly clear are not likely to confuse anyone who is prepared to read the book. Here are some of them: On p. 17 'differenten' is rendered by differentiated, which

does not express the meaning of the original and might mislead a careless reader. On p. 164, the German 'Kreuzweise gestellt' is rendered by 'placed crosswise,' which in English would nearly always be taken to mean 'placed across the long axis of the body,' though it might, in English, mean 'arranged in the form of a cross.' This is the meaning of the German.

On p. 191 the sentence beginning 'The embryo is now surrounded by' would be ambiguous if taken by itself, although it is entirely clear in the original. These instances and other similar ones are scarcely worth calling attention to in a work of such general excellence, and every zoologist has reason to be grateful to the translators for their self-sacrificing task.

The book is something more than a translation since both the authors and the translators have added to it numerous notes, which serve, for the most part, to call attention to the contributions that have appeared since the German editions were printed. These additions will be found particularly valuable to the specialist in directing his attention to the recent literature, but in most cases too brief to be of direct use to the student. The additions are distinguished from the original text by the use of brackets, and following each is an indication of its authorship. The authorship of these additions can be a matter of little interest to the readers of the book, and one could wish that the additions had been expanded and the text rewritten to accommodate them.

The translators have added to the lists of literature appendices, which include the literature which has appeared since the publication of the German edition and constitute a very important addition to the book. In matters of bibliography the papers issuing from the Zoological Laboratory of Harvard University have long been models and these appendices are no exception.

Finally the translators have added excellent indexes, subject and author.

The publishers have done their part of the work satisfactorily, and especially so with reference to the illustrations, most of which it would be difficult to distinguish from the originals.

JACOB REIGHARD.

Artistic and Scientific Taxidermy and Modelling. A manual of instruction in the methods of preserving and reproducing the correct form of all natural objects, including a chapter on the modelling of foliage. By MONTAGU BROWNE. London and New York: Macmillan & Co. 1896. $6.50.

It is something like twenty years since the appearance of Montagu Browne's Practical Taxidermy, a book of some 150 pages, and the present handsome volume of nearly thrice that size may be taken as representing the improvements in the art of taxidermy which the author considers to have taken place during the last twenty-five years. The book opens with a brief review of the origin and progress of taxidermy, next comes a short chapter on tools, and then follows a long and valuable section devoted to formulas for various killing, preservative, modelling and other compounds, most of which have been tested, and many of which have been devised by Mr. Browne. This chapter, which includes notes on the permanency of pigments, will prove most useful to both the amateur and professional preparator, for in it are brought together a host of recipes which, even when printed elsewhere, are scattered far and wide. Here, for example, are to be found many of the methods used in the preparation of the beautiful invertebrates sent out by the Naples Station, and here are formulas for making the gelatin casts which have come so much into vogue of late years. Few, however, will agree with Mr. Browne's wholesale denunciation of arsenic as a preservative, and fewer still will accept in its stead whiting and chloride of lime, much less pepper! Arsenic may be used with too free a hand, and exposure to light and air may go far towards preserving fur and feathers from the attacks of insect pests, but arsenic certainly prevents the ravages of Dermestes, and there is nothing like it for preserving intact ligamentary skeletons and the sterna of mammals. Especially is this true where hundreds of small 'rough skeletons' are of necessity kept in the duplicate series to be worked on as occasion demands.

From the chapter on collecting one infers that those useful articles, the auxiliary barrel and cyclone trap, have not found their way across

the Atlantic, but we pass on to the mounting and casting of animals which occupies the body of the book. It may be said here that the personal equation is quite as important a factor in taxidermy as in other matters, and while the author's advice and methods are mainly good there are many points wherein it is impossible to agree with him. Mr. Browne also takes it too much for granted that specimens are to be mounted fresh, whereas the majority of specimens which come under the hand of the taxidermist are dry skins, and only too often very poor dry skins. Hence more detailed instructions for relaxing and cleaning dried skins would have been acceptable. The various groups of vertebrates are treated in order, considerable space naturally being devoted to birds. Here the criticism would be that the advantages of skinning birds through the side are slight, the disadvantages numerous, and we would advise the taxidermist to open and mount his birds by a median cut. Also, we consider that mounting a bird with the entire skeleton inside is a great waste of valuable time; we have seen it tried, and the result did not justify the time and labor expended. In fact, the quality of the finished work depends not so much on the mechanical devices employed as on the artistic eye and skilled hand of the workman. The good taxidermist, any more than the artist, needs not to build upon a skeleton, although a knowledge of anatomy is indispensable to each.

The method of mounting mammals over paper casts is dwelt on with veritable enthusiasm, and although we have never seen it practiced, it would seem to be a most excellent plan for obtaining light and accurate specimens. The paper cast is certainly most admirable for copying cetaceans and large fishes, but the reader will not find it so simple in practice as it seems in theory, particularly if undertaken in a damp climate. Also it needs as much skill in this mode of mounting as in any other to avoid stiffness in posing.

The greater portion of the book, after the chapter on birds, is given over to describing various methods of moulding, casting or modelling fishes, reptiles, batrachians and invertebrates, and to the making of accessories, such as flowers, leaves and rockwork. This, supplemented by the recipes noted in the beginning, contains some of the most valuable information in the book, and will well repay study, since it treats of extremely useful technical processes which usually have to be learned from some expert. It is a pity, however, that in treating of flowers the reader is not told where he can obtain the oft-mentioned 'Mintorn fabric,' or, failing in this, advised to procure waxed cloth from some dealer in artificial plants, or in the materials for making them.

Finally, there is a very full bibliography of taxidermy, and last, but not least, an index.

From what has been said it will be rightly inferred that the value of this book lies not so much in the portion devoted to taxidermy proper as in that treating of other and related subjects; it can not supersede such a work as Hornaday's Taxidermy, but it is nevertheless indispensable to the preparator for its merits in other lines.

It may not be out of place to say that, to a great extent, the pages of this book reflect the changes that have taken place in museums during the past few years. The time was when the museum of natural history was almost wholly for the scholar, the cultivation of the public being quite a secondary consideration. Birds and mammals were represented by more or less poorly stuffed specimens, and anything of a pictorial nature, or even the replacement of colors or of soft parts, was religiously tabooed. Now it is recognized that at least one of the objects of a public museum is to give the public glimpses of living creatures as they really appear, and it is admitted that it is better to replace such appendages as combs and wattles, or even to obscure the scales of a bird's foot with paint, than to show the public dried, distorted and dingy effigies. The visitor does not care to count the scales on a snake's back nor the rays of a fish's fin, but he does wish to know how the living snake looks and in the gorgeous but evanescent colors in which so many fishes are decked. The shrivelled, faded and often imperfect spirit specimen may furnish taxonomic facts to the naturalist, but the public should have something else.

F. A. LUCAS.

U. S. NATIONAL MUSEUM.

SCIENCE

Friday, August 28, 1896.

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*A COMPLETED CHAPTER IN THE HISTORY OF THE ATOMIC THEORY.**

The great discovery of the law of gravitation was left reasonably complete by its author. The explanation of this fact is obvious. No other force of sensible magnitude complicates the action of gravitation; its law appeals to simple geometrical relations; and the facts had been well observed and reduced to order. Accordingly, by a few numerical comparisons of the hypothesis with the facts, Newton established the truth of his conjecture, so that it has been generally accepted as a law of nature. The first suggestion of the theory was quickly followed by its final triumph.

Very different has been the history of the discovery which most chemists regard as next in importance to that of Newton. The discovery that matter consists of an aggregation of infinitesimal units or individuals was made by Dalton; but the first suggestion of this kind had been made at least twenty-two centuries before Dalton. Leucippus and Democritus were the earliest recorded believers in this doctrine; Epicurus adopted it; Lucretius expounded it in strains of noble eloquence. But all the early suggestions were quite barren and unfruitful for the advancement of science, for no one before the present century was in a position to make any verifiable hypothesis; and science grows by means of hypotheses so closely in touch with facts as to be verifiable. In later times, Leibnitz accepted the notion of a certain kind of atomic structure of matter; Newton accepted, and reasoned soundly upon, a view which Dalton recognized as akin to his own. Kant

*Address by the retiring President of the American Association for the Advancement of Science at the Buffalo Meeting.

seems to have adopted the contrary opinion, and to have believed that matter is infinitely divisible. But Bernouilli made the conjecture, which has since been verified, that a given volume of gas consists of a very large number of very small discrete particles, which we now call molecules; and Higgins, an English chemist, a contemporary of Dalton, was the first to apply the notion of atoms to the explanation of chemical phenomena, although he did not think clearly in regard to the weight of atoms, and so formed no useful hypothesis. Accordingly the net result of twenty-two centuries of thought on this subject was to form a conception of a possible structure of matter, without imagining any way of establishing the truth or error of this conception, or even of gaining any evidence whatever in regard to it. But, if any are inclined to visit this failure with reproach, it is interesting to notice that the first man who was aware of the quantitative relations which are adapted to throw light on the matter did not fail to make the most full and complete use of this knowledge.

Dalton, and not the ancients, ought to be regarded as the discoverer of the atomic structure of matter, because he invented a hypothesis, involving such a structure, which was capable of being so compared with facts as to be proved or contradicted; because he actually began such a comparison of the hypothesis with the facts; and because all the evidence from facts, varied as it has since become, supports the hypothesis substantially in the form which he gave it. He who suggests that a certain benefit is desirable, or who conjectures that it is possible, shall not fail of due credit; but he who *confers* the benefit will receive the credit due the benefactor.

Since Dalton's discovery, much has been done to confirm and enlarge our knowledge of the atomic structure of matter. New evidence has been acquired in favor of it, because the theory has been ready to extend over whole realms of facts of a kind unknown to Dalton, to explain them, to facilitate their study; and also ready to predict facts, unknown till they were sought in consequence of the prediction, but found when they were sought.

The history of the atomic theory for ninety years would fall into several distinct chapters. One of these chapters, not the least interesting of them, would tell of a very large amount of work, some of it of consummate accuracy, of which the object was to attain some knowledge of the nature or construction of atoms. Since the last meeting of our Association in this city, work has been accomplished which, if I rightly judge, has ended this particular chapter. That the chapter may at some future time be resumed is, of course, not absolutely impossible; but for the present it has come to a definite close. My own interest in the matter suggests, and the coincidence in time now mentioned perhaps justifies, my selection of this completed chapter in the history of the atomic theory as the subject of the address which our constitution requires of me this evening.

This chapter naturally concerns more intimately the members of the sections of Physics and Chemistry. To these I can hardly hope to say anything not already well known to them; but members of other sections may, perhaps, not be entirely uninterested in an account of the conclusions reached.

Dalton's theory was founded on three facts. These facts are often called Dalton's laws; one of them, because he discovered it; the others because he first recognized their important relations to chemical theory. One of these is the law of definite proportions: in any chemical compound, the ratio of the components is constant, is invariable, is definite. This truth had been recognized by others; it was finally established as a

result of the discussion between Berthollet and Proust, a discussion well worth recalling for the dignified courtesy and simple love for truth shown by both the disputants. A second of these laws of Dalton is the law of equivalent proportions: if two elements, which combine with each other, combine also with a third, then the ratio in which they combine with each other (or a simple multiple of it) is also the ratio of the quantities of those which combine with the same quantity of the third. That this was true, at least in some cases, was known before Dalton. The third law is the law of multiple proportions: if two bodies combine in more than one ratio, those ratios are simple multiples of each other. This truth was discovered by Dalton.

These three laws are statements of *facts*. Careful and multiplied experiments have convinced us that, if these statements are not rigorously exact, their deviation from accuracy is less than the accidental errors of the best experiments used to test them.

Perhaps it is worth while to delay for a moment, in order to state to what degree of precision such experiments have been brought. The degree of precision with which any supposed law can be verified depends on the skill of the investigator, on the instrumental equipment available, and on the conditions of the problem. Often the conditions of the problem impose very stringent limitations on the precision of our experiments. For instance, the truth known as Ohm's law has been verified, in the case of metallic conductors, to one part in a million millions; but in the case of liquid conductors, the conditions are such that the precision attainable so far has been only a millionth as much. Huyghens' law, relating to double refraction, has been verified to one part in half a million, and there seems to be no possibility of attaining any considerable increase in the precision of the observations. These are examples of the very highest degree of precision which has been secured in the verification of supposed laws of nature.

The precision which can be attained in chemical analysis, even of the most elaborate kind, is much less than in the cases just mentioned. The determination of atomic weights is the chemical process in which the highest degree of precision is demanded. If we denote the precision of such determination by the words 'good,' 'excellent,' 'admirable,' 'consummate,' then we may fairly say that in a good series of determinations the average difference from the mean of all will be less than one thousandth part of the ratio sought; in an excellent series, less than one three-thousandth part; in an admirable series, less than one ten-thousandth part; and in a consummate series, less than one fifty-thousandth part.

Now the work of Stas was all admirable in precision, and much of it was consummate, and he made experiments expressly intended to verify the law of definite proportions. The average error in this series of experiments was not more than one part in thirty thousand; and his result was, that, if the composition of the compounds examined is not rigorously constant, the variations are too small to be detected. The law of equivalent proportions was verified with the same degree of precision; the accuracy of the law of multiple proportions has been thought to be deducible from the truth of the two other laws.

To some such degree of precision, then, Dalton's laws are the expression of facts. With these facts for a guide, and with no theory founded on the facts and explaining the facts, all chemical computations could be made, and chemical formulæ could be established. And, if a theory should be devised, and accepted, and finally overthrown, these facts would remain, unchanged for our perpetual guidance. Some of Dalton's contemporaries accepted the facts as a suf-

ficient guide, and refused to burden them with the weight of the theory. Some were engrossed, for the time, in following out practical consequences of the facts; some distrusted conclusions supported by but a single line of evidence; some, perhaps, distrusted the capacities of the human mind. But the facts were accepted.

All scientific men, all sensible men, have a great respect for facts. Perhaps one cannot have too great a respect for facts; but his respect may be wrongly directed. Facts are often very interesting in themselves; they often have an important relation to human welfare; their discovery is often a great intellectual triumph; and we may regard them as the miser regards his gold, forgetting that the most precious use of facts is to help us to see beyond them. Facts are evidence; but we seek a verdict. Facts are a telescope; we desire enlargement of vision, further insight into nature. Facts are openings which we laboriously hew in the walls which shut us in; they cost enough to be valuable, but their real value is in that which they promise or disclose. Facts are a foundation for our building; the structure must rigorously respect the lines of the foundation; but it is a pity to believe that the basement walls are the chief beauty desired by the architect or owner. As Tyndall phrased it in a lecture at Manchester, "Out of experience in science, there always grows something finer than mere experience. Experience, in fact, only furnishes the soil for plants of higher growth."

In the present case the soil was fertile, the finer growth has been rapid and vigorous. Dalton inferred that chemical elements consist of very small units or individuals; that all the units or individuals of any given element are equal in weight; and that combination takes place by the grouping together of different units or individuals. This is Dalton's atomic theory.

In Dalton's time there was no fact opposed to this novel conclusion; but there was no second set of facts to support it. The progress of chemistry depended on making due use of Dalton's three laws, and they were quickly and generally accepted; but whether the hypothetical chemical units or individuals actually exist or not, although a most interesting question, did not press for instant decision. Most chemists regarded with favor the idea of the actual existence of the chemical units or individuals. Dalton called them atoms, and perhaps the name brought misfortune; for many thought that the new theory was, that matter is made up of units or individuals which cannot be divided by any possible force. The word 'atom,' the word 'indivisible,' like the word 'individual,' properly mean that which is not divided in the phenomena considered. An absolutely indivisible atom, like an irresistible wave or an immovable rock, can be spoken of to puzzle children, but for adults, as Clifford said, "If there is anything which cannot be divided, we cannot know it, because we know nothing about possibilities or impossibilities; only about what has or has not taken place." I judge that many, probably most chemists and physicists understand the word atom correctly; many others understand it to mean that which cannot be divided by any possible force, and so misunderstand it. For instance, the author of the 'History of the Inductive Sciences' failed to understand the word as chemists and physicists understand it, and so supposed that he rejected the atomic theory. Many chemists would reject the theory that matter consists of very small units which *cannot* be divided. I suppose that very nearly all believe that matter is made up of small units which are not divided in any chemical or physical change yet observed. This is the atomic theory of Dalton.

A few years after Dalton had formed the

atomic theory, and had obtained the first experimental evidence on a matter which had enlisted attention for more than two thousand years, Davy showed, by brilliant experiments, that certain bodies were compounds, although they had resisted all previous attempts to decompose them. Since the first use of electricity had so important results, men were ready to suspect that even supposed elements might ultimately prove to be compounds. It was therefore in a congenial soil that Prout's hypothesis took root. Trusting to experiments of not much accuracy, Prout suggested, in the year 1815, that probably the atomic weights of other elements were divisible, without remainder, by the atomic weights of hydrogen; or, in other words, that they are whole numbers, if the atomic weight of hydrogen be taken as unity.

The new suggestion was most attractive, for two reasons: On the one hand, the truth of the new suggestion would lead to a very great practical advantage. The labor of determining atomic weights would be immensely simplified and lessened if we could know beforehand that the numbers to be found were integers. And, on the other hand, the new suggestion, if approved, would promise a most interesting and valuable hint as to the nature of matter and the structure of atoms. If, for instance, the atoms of carbon and nitrogen and oxygen weigh precisely as much as twelve and fourteen and sixteen atoms of hydrogen, then it is a very plausible hypothesis that each of these atoms is really composed of the material of twelve and fourteen and sixteen atoms of hydrogen, compacted into a new atom. Davy had led many to suspect that perhaps some atoms might be compound, and the new suggestion, looking in the same direction, was received with favor by many, among whom were great discoverers, and great experimenters, and great teachers of chemistry. In England, where Davy and Prout both lived, Thomson had great influence. It was Thomson who, in the *Journal of Chemistry*, of which he was the editor, first announced Dalton's discovery. Thomson wrote the history of chemistry. Thomson's 'System of Chemistry' was thought worthy of translation into French at a time when French was the mother tongue of chemistry. And Thomson accepted Prout's hypothesis as probably true. But Turner made more accurate and more numerous determinations of atomic weights than any other English chemist; and he rejected Prout's hypothesis. Berzelius, the great Swedish chemist, whose determinations of the atomic weights of all the elements then known were regarded with so much admiration by all chemists, pronounced Prout's hypothesis a pure illusion. But Dumas, than whom none in France stood higher, whose opinion had great weight on account of the excellence of his many determinations of atomic weights, accepted Prout's hypothesis with a slight modification, and believed that his experiments had established its truth. Stas, the distinguished pupil of Dumas, began his work with a bias in favor of the hypothesis; but when his first series of admirable determinations of atomic weights was published, he pronounced the hypothesis a pure illusion, entirely irreconcilable with the numerical results of experiment. But Mallet, who has made several excellent determinations of atomic weights, and Clarke, who has recomputed and reduced to order all the published determinations, declared themselves forced to give Prout's hypothesis a most respectful consideration. It is obvious, then, that ten years ago it was not finally settled whether the hypothesis was or was not true.

The hypothesis, then, has disappointed our hopes of any practical advantage in conducting to a knowledge of the exact value of any atomic weight. But neverthe-

less the hypothesis has not been neglected. As was said, if it is true, we may expect from it new insight into the nature of atoms. Accordingly, an immense amount of labor has been expended in attempting to determine whether the atomic weights of certain elements are or are not divisible without remainder by the atomic weight of hydrogen. Now since our last meeting in this city results have been attained which show that further effort in this direction is not justified by the hope of any theoretic advantage. The chapter has come to an end. Prout's hypothesis cannot be proved by experiment.

When we attempt to decide by experiment whether Prout's hypothesis is true, the nature of the problem, and the limitations of our present knowledge and of our available manipulative skill, impose three conditions to which we must conform.

In the first place, we can more readily test the correctness of Prout's hypothesis by determinations of the smaller atomic weights. The reason is obvious. All analytical work is affected with some accidental error or uncertainty. When Herschel wrote his admirable 'Discourse on the Study of Natural Philosophy' he said that it was doubtful whether we could depend on the result of a chemical analysis as having an uncertainty less than one part in four hundred. Work of much greater accuracy has been done since this statement was made; but, for the moment, let us assume that, even now, the uncertainty of a determination of an atomic weight is a four-hundredth part. This uncertainty affects a large atomic weight much more unfavorably for our purpose than it affects a small atomic weight. For instance, Stas found the atomic weight of lead to be 206.91, if we take the atomic weight of oxygen as 16.00. The assumed uncertainty, one four-hundredth part of this, is 0.53 ; so that, on our assumption, the true value is somewhere between 206.38 and 207.44. These numbers differ more than a unit; no one has a right, on this showing, to assert that true value is the whole number 207.00, nor that it is not so.

But a small atomic weight may be much less unfavorably affected by the same proportionate uncertainty. For instance, recent determinations show that the atomic weight is 15.88 when the atomic weight of hydrogen is taken as unity. The assumed uncertainty, one four-hundredth part of this, is 0.04; so that, on our assumption, the true value is between the limits 15.84 and 15.92. These numbers differ by only one twelfth of a unit; and both of them differ much from the nearest whole number, 16.00. It is, therefore, by determinations of small atomic weights that we may hope to decide the truth of Prout's hypothesis.

But among the smaller atomic weights, some, in the present state of our knowledge, can be more accurately determined than others. Accordingly a second condition imposed on us by the limitations of our knowledge is that we must determine, with what precision we can, those small atomic weights which admit of the maximum of precision. There are eight atomic weights upon which, with the experimental data now available, the decision of the matter may be fairly made to depend. These elements are lithium, carbon, nitrogen, oxygen, sodium, sulphur, chlorine and potassium; the atomic weights are, in round numbers, 7, 12, 14, 16, 23, 32, 35.50 and 39. If numerous and careful experiments show that these atomic weights are whole numbers Prout's hypothesis has a solid basis in fact; if seven are whole numbers and the other is 35.50, then Dumas's modified statement of the hypothesis has a solid basis in fact, for 35.50 is divisible without a remainder by *half* the atomic weight of hydrogen.

One more condition is imposed on us by the limitations of our knowledge and

manipulative skill. Our experiments determine most atomic weights, not with reference to hydrogen, but with reference to oxygen. Experiment, for instance, does not determine directly that the atomic weight of lithium is seven times that of hydrogen, but that it is seven sixteenths that of oxygen. If the atomic weight of oxygen is uncertain, the atomic weights of the other seven elements, with reference to hydrogen, are all uncertain in the same proportion, although with reference to oxygen they are now determined with very small uncertainty. Accordingly the third condition imposed on us in attempting to learn the truth about Prout's hypothesis is that the atomic weight of oxygen must be well determined.

It may be remarked that it would be a great gain, as all chemists will see, if several other atomic weights could be determined by direct comparison with hydrogen, provided the precision attainable was of the degree which I have called admirable, or even excellent. Now, methods have been devised by which the atomic weights of lithium, sodium and potassium, as well as of several other metals, could be referred directly to hydrogen, by experiments which present no great difficulty and which are capable of the required precision. Further, a method has been devised by which the atomic weight of chlorine can be determined with direct reference to hydrogen, by experiments capable of the required degree of precision, but involving considerable difficulty in manipulation. But, until some such methods shall have been employed by some one, we must be content with the inferences which can be drawn from data of the kind now available, which depend on our knowledge of the atomic weight of oxygen as the corner stone of the system.

Our knowledge of the atomic weight of oxygen ten years ago depended largely on the experiments of Dumas. His results differed from the whole number 16.00 by one four-hundredth part; he himself judged that the uncertainty remaining might be one two-hundreth part. If we accept this estimate of uncertainty, we may say that he proved that the atomic weight of oxygen is included between the limits 15.88 and 16.04. No one could assert that the true number is, or that it is not, the whole number 16.00. A proportionate uncertainty, therefore, existed in the other seven atomic weights just mentioned. Accordingly, ten years ago we could not well discuss the question whether these atomic weights were divisible, without remainder, by the atomic weight of hydrogen.

The atomic weight of oxygen is, accordingly, doubly important for our purpose. The atomic weight is a small one, well adapted to aid in the solution; and, further, many other atomic weights, also well adapted to aid in the solution, depend on a prior knowledge of this constant. It is for this twofold reason that the work done since our last meeting at Buffalo is important and interesting. The members of this Association have not failed to take upon themselves a fair proportion of the considerable labor involved.

Since that time not less than ten or eleven independent determinations of the atomic weight of oxygen have been successfully concluded.

Cooke and Richards were the first to complete and publish their result; they used a new and ingenious process. Keiser was next; he employed a method for weighing hydrogen which he had independently invented (though it had been previously invented elsewhere) which is the best yet used. In both these series of experiments the hydrogen was combined with oxygen by manipulation something like that of Dumas; but the improvement which permitted the direct weighing of the

hydrogen made the essence of the process novel. Then Noyes devised a new method of weighing hydrogen directly, and a new manipulation for combining it with oxygen, and carried out the process in an apparatus having the advantage of great simplicity. Further, since our last meeting the Smithsonian Institution has published a work containing three series of determinations of the value in question.

In England, Lord Rayleigh used another novel method of combining oxygen and hydrogen, in which he weighed both elements in the form of gas. He also made two series of determinations of the ratio of the densities of the gases. Scott determined the ratio of the volumes of the gases which combine, in several series of experiments of great accuracy. Dittmar and Henderson rendered an important service by repeating, with many modifications, the experiments of Dumas; with the advantage which the later experimenter commonly has over the earlier, they were able to secure a much higher degree of precision and to eliminate the sources of constant error which Dumas detected too late.

In France, Leduc repeated the experiments of Dumas and also determined the ratio of the densities of the two gases.

In Denmark, Thomsen has applied a different process, in which the atomic weight of a given metal is compared with those of oxygen and of hydrogen successively.

We have, then, eleven series of determinations of the atomic weight of oxygen. One of these, for reasons which, so far, are chiefly matter of conjecture, differs much from the mean of all the others. These other ten are concordant; they differ, on the average, only one part in twenty-two hundred from their mean, and the greatest difference from the mean is about one part in a thousand.

Since these experiments have been made by different processes, by different men, under varied conditions, and since the greatest difference from the mean of the whole is only one part in a thousand, it is probable that the mean of all differs from the truth by much less than one part in a thousand. The errors of our experiments are of two kinds—accidental and systematic. If we shoot a hundred times at a mark, about half of our shots fall a little to the right and about half a little to the left. These are accidental errors; accidental errors are lessened as our manipulation improves, and they but slightly affect our final mean. Systematic errors affect all our results in the same direction. Suppose we fire a hundred shots at a target one thousand yards distant, not examining the target, till the shots are all fired. If, now, the sights of our rifle were set for five hundred yards, all our shots would strike too low. This is a systematic error; systematic errors diminish as our knowledge increases.

Accidental errors can be rendered harmless by taking the mean of numerous determinations made by the same method. But systematic errors must be detected and avoided. That they have been detected and avoided in any given case can never be definitely known; it can, at best, be presumed from the fact that experiments by different methods give the same result.

As to the atomic weight of oxygen, accidental errors have now been fairly eliminated, and we can make definite numerical statements on this point. If each of the ten sets of experiments were to be repeated, with the same skill and knowledge, there is not one chance in a thousand that the new mean would differ from the present mean by as much as one part in sixteen thousand. Again, if ten new sets of experiments were to be made by new methods and new experimenters, there is not one chance in a thousand that the new mean would differ from the present mean by as much as one part in twenty-five hundred.

As to possible systematic errors, modesty in statement is incumbent upon all scientific men. But we have now ten independent results in which the difference from the mean is at most only one part in one thousand. We may then fairly assume that the systematic error of the mean is less than one part in one thousand. Again, we have lately been able to take one step in advance, which throws needed light on precisely this point. It has been found possible to weigh some hydrogen, to weigh the requisite oxygen, and to weigh the water which they produce. If, now, there were some undetected systematic error in weighing either one of these three substances, occasioned, for instance, by some impurity remaining undetected in one of them, the sum of the weights of the hydrogen and oxygen would differ from the weight of the water produced. If a pound of sugar and a pound of water produce only one pound and three quarters of syrup, there was a quarter of a pound of sand in the sugar. Now it has, I think, been proved that, if the sum of the weights of the hydrogen and the oxygen is not precisely equal to the weight of the water produced, the difference is too small to be detected, and cannot be more than one part in twenty-five thousand. If there really were a difference of this amount, and, further, if this difference were due to an error at the precise point where it would be the most mischievous, it would render the atomic weight of oxygen uncertain by one part in about twenty-eight hundred.

Taking into account the presumption from the concordance of the results of different experimenters and the presumption from the agreement just mentioned, I think we are justified in assuming that the remaining systematic error is not more than one part in sixteen hundred, and that it probably is not more than one part in three thousand.

If this is a reasonable assumption, the net results of the experiments made in Denmark, France, Great Britain and the United States is that the atomic weight of oxygen is between 15.87 and 15.89, and that probably it is between 15.875 and 15.885. By no stretch can we imagine that the truth lies in the whole number 16.00, nor in the even fraction 15.50. We cannot sanely believe it to lie in the number 15.75, having modified Prout's hypothesis into the new statement that all atomic weights are divisible, without remainder, by one *quarter* of the atomic weight of hydrogen. It will be obvious that, if we are still resolved to accept some form of the attractive illusion, we must assume that the true divisor is as small as one eighth of the atomic weight of hydrogen, for the value $15\frac{7}{8}$ is included within the limits given.

Then there is one small and well determined atomic weight which utterly refuses to support Prout's hypothesis or any modification yet stated by believers in the hypothesis. Further, now that the atomic weight of oxygen is well established, we can compare, with hydrogen taken as unity, the seven other small and well determined atomic weights which have been mentioned.* We see that every value differs from an integer; for lithium, nitrogen and potassium the difference is about one part in two hundred thirty; for sodium, sulphur and chlorine, about one part in one hundred eighty; for carbon and oxygen, about one part in one hundred thirty. On the average, these values, which are the best determined in chemistry, differ from whole numbers by about one part in one hundred eighty. There is less than one chance in a thousand that these numbers can possibly be so much in error. These are the numbers best fitted to test Prout's hypothesis, and their evidence against it is decisive.

* The values are as follows: Li=6.97, C=11.91, N=13.94, O=15.88, Na=22.87, S=31.83, Cl=35.19, K=38.84.

It ought to be added that the evidence against Prout's hypothesis seemed to many to be decisive, even without the knowledge of the atomic weight of oxygen which has recently been acquired. But the evidence can now be stated in a much more direct and simple manner; and it has gained in force, for to the seven fit instances at hand before there is added an eighth, which happens to be the most weighty of the whole.

In order to present the evidence against Prout's hypothesis when we lack an accurate knowledge of the atomic weight of oxygen, we have first to assume this value. We may, for one trial, assume that this value is the whole number 16.00, which is required by Prout's hypothesis, and see whether, on this assumption, the other seven atomic weights in question are very nearly such as the hypothesis requires.* But the average deviation from the numbers required by the hypothesis is one part in five hundred, and one deviation amounts to more than one part in three hundred. We may make another trial by assuming for oxygen, not the whole number 16.00, but that value which shall make the sum of all the deviations the least possible; and we may also take one quarter of the atomic weight of hydrogen as our divisor.† But the average deviations from the numbers required by the theory is, even in this case, one part in six hundred and the atomic weight of that element for which the determinations of friends of the hypothesis agree with those of its opponents to one part in thirty-five hundred, is supposed, after all, to be in error by one part in five hundred. The atomic weight of oxygen, computed expressly to give every possible advantage to the hypothesis, differs from the whole number required by the theory by one part in two hundred fifty.

We read in our school books of the bed of Procrustes, to which the tyrant fitted his compulsory lodgers; if they were too short he stretched them on the rack; if they were too long he lopped off the superfluous length. This fable was really a prophetic vision; the bed is Prout's hypothesis; our friends who admire it want to stretch the most unyielding quantities, and to lop off numbers which have been determined with the greatest precision. Either the experiments are in error by an amount which seems incredible, or the hypothesis is an illusion. If the supporters of the hypothesis would avoid the conclusion they must supply better determinations, or they must detect real and tangible sources of error in those already made.

The hypothesis was most interesting and attractive; it promised, if sustained by experimental evidence, to give the means of such insight into the nature of the matter and into the intimate structure of the atoms that it was well worth all the attention which has been given to it. That it should fail of support, that its promises could not be kept, is a matter of regret; but it is time to recognize that our hopes are quite cut off. That other elements are composed of the same substance as hydrogen may or may not be true, but we have now no hope of proving it by determinations of atomic weight. It would not be difficult, perhaps, to modify Prout's hypothesis again and again, so as to to bring it into some accord with the facts. We may imagine, if we will, that the observed numbers, if determined without error, would all be divisible by the eighth part of the atomic weight of hydrogen, or the ninth, or the tenth, or by some smaller fraction. But such a hypothesis is of no interest and of no utility, be-

* The values on this assumption are as follows: Li=7.02, C=12.00, N=14.04, O=16.00 (assumed), Na=23.07, S=32.04, Cl=35.46, K=39.14.

† The values are as follows: Li=7.00, C=11.96, N=13.99, O=15.94, Na=22.96, S=31.96, Cl=35.33, K=39.00.

cause it is incapable of proof or disproof by experiment. The reason is obvious. If we suppose that all atomic weights are divisible by one tenth of the atomic weight of hydrogen, then, in case the theory is erroneous, the average deviation of the actual atomic weights from those required by the theory is only the fortieth of the unit. The man who supports a theory which has no physical basis would assert that all such ascertained deviations were due to errors of experiment. Others would reply that you cannot prove that a man is a good marksman by crowding the targets so near each other that not even his random shots can miss them all. But his backers might make so uncritical a claim.

No, Prout's hypothesis, if subdivided far enough, may be true for all which can be proved with the balance; but in such new form it is of no use and of no interest, for it cannot be proved so as to become a safe basis for further inference. In its present form there is no root of truth in it.

So far I have argued that Prout's hypothesis is not true as heretofore enunciated, and that, if some further modification of it is true, we cannot know it. This conclusion has been sustained by the evidence of the chemist's balance. A conclusion supported by a single kind of evidence may command the confidence of one who has been long familiar with the evidence and who has become capable of weighing it. But for others the concurrence of evidence of different kinds rightly adds greatly to its cogency. In this case there is such concurrent evidence. There is other proof that the atoms of some well studied elements are not *additive* structures. Let me briefly describe the nature of this evidence.

When certain elements are volatilized in a colorless gas flame, or in the electric arc, their molecules are made to vibrate, so as to produce light. By the study of this light we can in time learn much of the nature of the vibrating system. The observed facts are gradually reducing to order, and one result is very striking. In the case of three closely similar elements before mentioned, lithium, sodium and potassium, the complexity of vibration is precisely similar in all, and the numerical relations among the component vibrations are precisely similar in all. Therefore we are compelled to assume that the complexity of structure is the same in all, and that the relations of the component parts, and of the forces acting between them, are the same in all. To illustrate the nature of the argument: the complexity of vibration and the numerical relations among the component vibrations in the case of a large church bell are precisely similar to those in the case of a bell only one third as large. Then, even without the direct evidence of other senses, we must presume that the two bells are similar structures, having similar parts, similarly related. We cannot believe that the larger bell is made of a small bell loaded with weights, nor of three small bells bound closely together. The larger and the smaller are of the same order. The larger is not made of more *parts* than the smaller; it is made of more *metal*. So with the atoms of these three elements; the larger are not made up by the addition of parts which preserve their identity and remain undivided. But all we know of chemical combination relates to structures which are made by the addition of parts which preserve their identity and remain undivided. Then Prout's hypothesis assumes an analogy which does not exist; and deductions from an imaginary analogy will themselves differ from the truth, much as fairy tales differ from history.

There are still other sources of evidence drawn from the specific heats of the elements; the evidence is of the same kind and leads to the same conclusion, but I simply allude to it.

It seems to me, then, that the exact quantitative similarity of the spectra of these elements shows that they are not compounds one of another, subject to the great chemical law of the addition of undivided parts; and that also the magnitudes of the small and well determined atomic weights differ from the values hitherto suggested by applying the law of the addition of undivided parts, and differ by five, ten and fifteen times the greatest experimental error we can reasonably assume.

So the citadel which defends the secret of the atom cannot be taken by way of Prout's hypothesis. We have carried on the assault for eighty years, and we are now satisfied that the way is blocked; we tried to breach, not a wall, but the solid mountain itself. We shall doubtless learn the structure of the atom, but we cannot learn it in the way we hoped. This chapter in our study of the nature of atoms has been fully ended.

If Prout's hypothesis cannot serve us you will doubtless ask what other ways are open by which we may learn something of the structure of atoms. To answer is difficult; to answer adequately is impossible. Perhaps I may mention four lines in which it has been hoped by some that the desired advance could be made, and may indicate what it is reasonable to expect of each.

One of these indications of a possible source of knowledge as to the structure of atoms was suggested by certain chemical observations on some of the rare earths. My brief explanation will not do justice to the conception of the eminent chemist who investigated the phenomena. As I have said, the atom is something which, as a matter of fact, remains undivided in all chemical changes. Most atoms seem to resist every force which we can apply. But it is possible that the amount of resistance which they can offer may vary greatly; it may be that in the case of some elements the resistance is such that in some reactions the atoms remain undivided, and not in others. From the study of such cases, if there are such, we might expect much help. Now, in the case of the common and well studied elements, the occurrence of such cases has not been suspected; but some of the rarer elements, examined by a process which is frightfully laborious, have exhibited phenomena which suggest, as a hypothesis to be further studied, such a subdivision of atoms. But it is probable that we have mixtures of distinct elements which we do not yet know how to separate from each other by simple analytical processes. This chapter, we may fairly presume, will be valuable; but not because it will tell us anything new about the structure of atoms.

Certain spectroscopic phenomena have suggested that some elements may be decomposed by the action of a high temperature. For instance, it has been thought not impossible that, at the temperature of the electric arc, potassium compounds quite free from sodium should begin to show the spectrum of sodium, because at this temperature potassium is decomposed so as to produce sodium. This hypothesis has been carefully investigated; in part, by the accomplished physicist who is its author; in part, at his suggestion and invitation. It is found that, if years are given to the preparation of potassium compounds free from every trace of sodium, then it is impossible to obtain from them any phenomena suggesting a decomposition into sodium. Here, again, the new chapter, as far as it relates to the structure of the atom, is likely to be but short.

A third suggestion did not rest upon any observed chemical phenomena, but was a purely intellectual creation. This is the hypothesis that atoms are vortex rings in a frictionless fluid. It belongs to the mathematical physicist, rather than to the chemist, to discuss this interesting sugges-

tion. It may be said that it has seemed not impossible that the chemist should find a vortex ring capable of exerting certain chemical forces. But the fate of the hypothesis rested, not with the chemist, but with the mathematical physicist; and it has been found that the theory demands that the weight of a body composed of vortex atoms should increase with rise of temperature. It is scarcely possible that this can be the fact; if, then, the mathematical and physical reasoning involved is sound, it is scarcely possible that atoms consist of vortex rings. The probability is, therefore, but small that we are to learn of the nature of atoms by means of this hypothesis.

Some spectroscopic and other optical phenomena seem to promise more light as to the structure of molecules and atoms, though the dawn is not yet. Thanks to the concave grating, we can determine the frequency of vibration of the light from any source with great accuracy. When the light is complex we can determine, with great accuracy, the relative frequency of the component vibrations. In the cases which have been best studied, the observed frequencies have been reduced to rather simple numerical relations. From the study of these relations we may expect, in time, to determine the structure of the vibrating systems. But the way is long and difficult. Let us illustrate the nature of the method by means of a familiar example, namely, by the study of the structure of a sonorous vibrating system by means of the study of the sonorous vibrations produced by it.

Let us suppose a person deprived of the sense of hearing, but master of the whole mathematical theory of sound. Suppose, further, that he has an instrument which will do for sound what the spectroscope will do for light. With this instrument, let him observe the frequency and the relative intensity of the vibrations produced by certain musical instruments which we cause to vibrate for him, but withhold from his inspection. Let us, first, sound for him a single note on a piano. The vibrations produced are, as you know, somewhat complicated. Our imagined experimenter, with his instrument, observes vibrations whose frequencies are 100, 200, 300, 400, 500 and 600 in one second; and he also observes that the vibrations of 100 and 500 are of nearly equal intensity, that the vibrations 200, 300 and 400 have more than twice as great an intensity, and that vibration 700 is very feeble. From these facts, if his attainments are sufficient and his imagination sufficiently fertile, he can determine what system produced the sound. He imagines every possible vibrating system—drum, cymbals, trumpet, flute, organ-pipe, harmonium-reed, violin-string, piano, harp and more. Next, assuming each imagined system of such size or tune as to produce one hundred vibrations a second for its gravest tone, he computes what other vibrations will also be produced and what the intensity of each. He finds, for instance, that a closed organ-pipe will give only the frequencies 100, 300, 500, but will not produce the other observed frequencies 200, 400, 600. Therefore, he concludes, the sound we produced for his study is not due to a closed organ-pipe. He finds, after many trials, that the observed frequencies and intensities could be produced by striking a stretched cord with a soft hammer, at a definite point near the end of the cord, so quickly that the cord and hammer remain in contact about the six-hundredth part of a second, and that the observed phenomena could not be produced by any other of the imagined vibrating systems. Then he concludes that the observed sound was probably produced by the stretched cord of a piano. He will have detected the true system, by first imagining every possible

system, by computing the frequencies and corresponding intensities due to each hypothetical system, and by then comparing computation and observation.

For a second example, suppose we ring, for our imagined observer, a bell of a certain form, and that he notes the frequencies 200, 475, 845 and 1295 in one second; in which, also, he finds that the vibration 845 so predominates as to give its pitch to the compound tone. Our observer will not be able to refer this sound to any stretched cord, or to any organ-pipe or other wind instrument; for all these are limited to frequencies contained in the series 200, 400, 600, 800. A uniform metallic bar, suspended and struck like the triangle of an orchestra, will give frequencies not contained in this list, but they will be 200, 550, 1080, and 2670, instead of 200, 475, 845 and 1295. But if our observer has adequate powers he will imagine a hemispherical bowl of suitable dimensions, and will, in imagination, add mass and rigidity in suitable places, until, in time, he will have devised a system whose computed vibrations agree in frequency, and in distribution of energy, with those of the invisible sounding body. Then he would conclude that the observed sound was due to a bell of the form assumed in the successful computation.

This illustration sketches, imperfectly, I fear, the laborious method by which we may learn the structure of a vibrating system from a study of the vibrations produced by it. When we attempt to use this method in order to learn something about the structure of molecules and atoms, our powers of imagination and our mathematical skill are none too much. We know but little which can suggest plausible hypotheses. The facts which are to be explained have been but recently reduced to order. Accordingly, little has been actually accomplished. But there are some few examples of the use of this method of studying the structure of molecules and atoms.

In one such example the structure imagined consisted of a system of concentric spherical shells, each connected with the adjacent shells by springs. This complicated structure admits of relatively simple computation, and was taken because it fairly well represents a rather simple imagined structure, for which, however, computation is difficult. But it was found that the results computed on this hypothesis gave little promise of agreement with facts.

This was a dynamical hypothesis; it suggested, not only vibrations, but the forces which were to produce them. A second example suggests certain possible motions, but not the forces which might produce the hypothetical motions; it is not dynamic, but kinetic.

As we know, many of the lines in the spectra of the elements are double. For instance, when a volatile compound of sodium is brought into a colorless gas flame, this is colored yellow. When we examine this yellow flame with a spectroscope of sufficient power, we see that there are two frequencies, differing from each other by only one part in a thousand. Now it is probable that these two frequencies are due to the vibrations of one and the same body. There are many illustrations of the fact that a given body may perform two different vibrations whose frequencies differ but slightly. For instance, if we suspend a ball by means of a cord and let it oscillate as a pendulum it is well known that a swing of six feet takes a little more time than a swing of three feet. Suppose, then, that we let our ball swing six feet north and south, and also three feet east and west at the same time; the two motions may be combined so that the ball moves in an ellipse—an ellipse whose longer axis is north and south. If the longer and the shorter swing had precisely the same frequency, the axis of the

ellipse would continue in this direction; but since the frequencies differ, the ellipse slowly revolves. Conversely, from the revolution of an ellipse, we should infer a difference of frequency in the two component vibrations. So it is suggested that the two slightly different frequencies in the light sent out by ignited sodium are due to an elliptic motion in the molecule in which the elliptic orb slowly revolves; this suggestion has not yet been carried so far as to specify any hypothetical cause for the revolution of the ellipse.

These two examples, both due to eminent English physicists, may serve to illustrate the method by which, if I am not mistaken, we are not unlikely to learn much as to the structure of molecules and atoms. We must not expect rapid progress. Even comparatively simple hypotheses may require, for their due examination, the invention of new mathematical methods. And useful hypotheses are rare: like the finding of buried treasures, they are not to be counted on. But, since Prout's hypothesis has rendered us its final service, new hypotheses must be devised, competent to guide us further on our way. Let us hope that, before this city again honors our Association with its invitation to meet here, American chemists and physicists may have had some honorable share in such new advance.

EDWARD W. MORLEY.

CLEVELAND, O.

PAST AND PRESENT TENDENCIES IN ENGINEERING EDUCATION.*

THE present status of engineering education in the United States is the result of a rapid evolution which has occurred in consequence of opinion as to the aims and methods of education in general. These changes of opinion, whether on the part of the public or on the part of educators, together with the resulting practice, may be called tendencies. All progress that has occurred is due to the pressure of such views or tendencies; hence a brief retrospect of the past and contemplation of the present may be of assistance in helping us to decide upon the most advantageous plans for the future.

Thirty years ago public opinion looked with distrust upon technical education. Its scientific basis and utilitarian aims were regarded as on a far lower plane than the well-tried methods of that venerable classical education whose purpose was to discipline and polish the mind. What wonderful changes of opinion have resulted, how the engineering education has increased and flourished, how it has influenced the old methods, and how it has gained a high place in public estimation are well known to all. The formation of this Society in 1893, its remarkable growth, and the profitable discussions contained in the three volumes of its transactions, show clearly that technical education constitutes one of the important mental and material lines of progress of the nineteenth century.

Engineering courses of study a quarter of a century ago were scientific rather than technical. It was recognized that the principles and facts of science were likely to be useful in the everyday work of life and particularly in the design and construction of machinery and structures. Hence mathematics was taught more thoroughly and with greater regard to practical applications, chemistry and physics were exemplified by laboratory work, drawing was introduced, and surveying was taught by actual field practice. Although engineering practice was rarely discussed in those early schools, and although questions of economic construction were but seldom brought to the attention of students, yet the scientific spirit that prevailed was most

*Presidential Address before the Society for the Promotion of Engineering Education at the meeting in Buffalo, N. Y., August 20, 1896.

praiseworthy and its influence has been far reaching.

This scientific education notably differed from the old classical education in two important respects: first, the principles of science were regarded as principles of truth whose study was ennobling because it attempted to solve the mystery of the universe; and second, the laws of the forces of nature were recognized as important to be understood in order to advance the prosperity and happiness of man. The former point of view led to the introduction of experimental work, it being recognized that the truth of nature's laws could be verified by experience alone; the latter point of view led to the application of these laws in industrial and technical experimentation. Gradually the latter tendency became far stronger than the former and thus the scientific school developed into the engineering college.

The very great value of laboratory experiments, and of all the so-called practical work of the engineering school of to-day, is granted by all. Principles and laws which otherwise may be but indistinct mental propositions are by experimentation rendered realities of nature. The student thus discovers and sees the laws of mechanics, and is inspired with the true scientific spirit of investigation. It should not, however, be forgotton that if such practical work be carried beyond the extent necessary to illustrate principles it may become a source of danger. The student of average ability may pass a pleasant hour in using apparatus to perform experiments which have been carefully laid out for him, and yet gain therefrom little mental advantage. Especially is this true when the work assumes the form of manual training, which, however, useful in itself, is properly considered by many as of too little value to occupy a place in the curriculum of an engineering college.

The tendency toward the multiplication of engineering courses of study has been a strong one, especially on the part of the public. This has resulted in a specialization that, as a rule, has not been of the highest advantage to students. In some institutions this has gone so far that the student of civil engineering learns nothing of boilers and machines, while the student of mechanical engineering learns nothing of surveying or bridges. The graduate is thus too often apt to lack that broad foundation upon which alone he can hope to build a successful career.

The development of the scientific school into the engineering college has been characterized throughout by one element of the happiest nature, that of hard work and thoroughness of study. The numerous topics to be covered in a limited time, their close interrelation, and the utilitarian point of view, have required many hours per week and earnest work by each student in preparation for each exercise. The discipline of hard and thorough work is one whose influence can be scarcely overestimated as a training for the duties of life, and in every university it is found that the activity and earnestness of the neighboring students is a source of constant stimulus to those of other departments. Thus scientific and engineering education has tended to elevate the standard and improve the methods of all educational work.

The length of the course of study in engineering colleges has generally been four years, and whatever tendencies have existed towards a five-years' course have now for the most part disappeared. With higher requirements for admission, particularly in English and in modern languages, a reduction of the length of the course to three years may possibly be ventured in the future, particularly if the long summer vacation be utilized for some of the practical work, as indeed is now the case in several institutions.

There has been and now is a strong tendency toward a reduction in the length of the college year. While formerly forty or forty-two weeks were regarded as essential, the process has gone on until now some colleges have but thirty or thirty-two weeks, a reduction of nearly twenty-five per cent. having been effected in twenty-five years. Undoubtedly the long vacation is utilized to great advantage by the majority of students in actual work, yet the fact remains that it is not good business economy to allow the buildings and plant of a college to lie idle for so large a part of the year. It is perhaps possible that in the future the summer schools may be so developed that the work will be practically continuous throughout the year, thus giving to students the option of completing the course either in three or four years.

The report of the committee on requirements for admission, which will be presented later in the session, sets forth many facts which show the tendencies now existing. Almost without exception a higher standard is demanded, both that students may enter with better mental training and that more time may be available in the course for technical subjects. While the general line of advance is toward an increase in mathematics and in modern languages, there is also found, particularly in the central states, a demand for broader training in science. It has already been pointed out that our early engineering schools were strong in scientific training, and that the tendency has been to replace this by industrial applications. If the requirements for admission can be extended to include the elements of chemistry and physics, with some botany or zoology, the engineering student will enter with broader views, a keener power of observation and a scientific spirit that will greatly increase his chances for success in technical studies.

The general increase in requirements for admission tends to raise the average age of the student. It is now usually the case, owing to the greater length of time needed in preparatory work, that the average age of the classical student is one year higher than that of the engineering student; or the former has had one more year of training than the latter. One more year of training means much as an element for success; one more year in age means an increase in judgment which is of the highest importance for a proper appreciation of the work of the course. The older men in a class usually do the best if not the most brilliant work, and after graduation their progress is the most satisfactory. It thus appears that all tendencies that raise the age of entrance are most important ones and deserve hearty encouragement.

Having now considered some of the general elements and tendencies in engineering education it will be well to take up the program of studies, especially in regard to those subjects that are common to all technical courses. The three volumes of the Transactions of this Society contain many carefully prepared papers and interesting discussions which enter into questions of detail concerning nearly all topics in the curriculum. Here, however, can only be noted briefly the main lines of development and the indications for future progress.

Mathematics is undoubtedly the most important subject in all courses of engineering study, and it has been demanded for years that it be taught with great thoroughness. This demand has been not more completely in the independent engineering colleges than in the engineering courses of the universities. Much, however, remains to be done in this direction, and probably it cannot be satisfactorily accomplished until a change in method has been effected. The fundamental element in the change of method must be, it seems to me, in a partial abolition of the formal logic of

the text-books and an introduction of historical and utilitarian ideas. Mathematics is a tool to be studied for its uses, rather than for its logic or for discipline that it can give; hence let its applications be inculcated frequently and not be systematically kept out of view. If the student gains the impression that his mathematical exercises are merely intended to train the mind his interest and his progress will usually be slow. If, however, he learns what mathematics has done in the past, how it joins with mechanics to explain the motions of the distant planets as well as to advance the material prosperity of man, there arises an interest and a zeal that helps him to overcome all difficulties.

The great advantage of numerical exercises in all branches of pure and applied mathematics, and the deplorable lack of good preparation in arithmetic, have been expressed by many educators. In numerical computations the average engineering student is weak in spite of the numerous exercises in his practical work. To remedy this defect better instruction in arithmetic is demanded in the common and high schools, while in engineering colleges the teachers of mathematics should constantly introduce numerical work and insist that it be done with a precision corresponding with the accuracy of the data.

Next in importance to mathematics comes mechanics, the science that teaches the laws of force and motion. In most institutions the rational is separated from the applied mechanics and often taught by the mathematical department. Probably less improvement has resulted in the teaching of rational mechanics during the past quarter of a century than in any other subject. That mechanics is an experimental science whose laws are founded on observation and experience is often forgotten, and the formal logic of the text-books tends to give students the impression that it is a subsidiary branch of mathematics. The most interesting history of the development of the science is rarely brought to the attention of classes, and altogether it appears that the present methods and results are capable of great improvement.

It should not be overlooked, however, that in recent years the so-called absolute system of units has been introduced into mechanics and is now generally taught in connection with physics. Here the pound or the kilogram is the unit of mass, while the unit of force is the poundal or the dyne. Although this system possesses nothing that is truely absolute, it has certain theoretical advantages that have commended its use, notwithstanding that no practical way of measuring poundals has been devised except by the action of the force of gravity on the pound. Engineers have continued to employ the pound weight as the unit of force, and the calculations of the physicist must be translated into the units of the engineer before they can be understood. The student of rational mechanics thus has the difficulty at the very outset of two systems of units, and great care should be taken that each be thoroughly understood and the relations between them be clearly appreciated by application to many numerical problems. In view of these and other difficulties and of the novelty of the subject in general, it appears that some engineering colleges do not give to rational mechanics as much time as its importance demands.

Physics in some colleges is taught by a course of five or six exercises per week, extending over a year, while in others the elements are required for admission and the regular course is correspondingly abridged. The marvelous development of electrical theory and practice has naturally tended to make this the most important topic in the course, sometimes indeed to a material abridgment of mechanics, acoustics, thermodynamics and optics. Considering

how great is the importance of each branch of physics and the advances that are made every year in new directions, it may also be concluded that more time can be profitably given both to theory and to experimental work. Physics is a fundamental subject whose principles and results are of constant application in every walk of life, and a student who thoroughly covers a well arranged course has gained a mental discipline and a scientific habit of mind that will be of greater value than the technical details of a purely engineering specialty.

Undoubtedly the most powerful tendency in engineeeing education has been in the direction of the development of those special technical subjects which may be grouped under the name of Construction and Design. In civil engineering this has led to plans for railroad, water supply and bridge construction ; in mechanical engineering to engine and machine design; in mining engineering to projects for mine plants, and in electrical engineering to the design of dynamos and motors. These courses have been demanded by the public and by the students themselves, and have been often elaborated to an extent beyond the best judgment of teachers of engineering. To the extension of such courses there is no limit, but it is a question whether the process has not already gone too far. For instance, it would not be difficult to arrange a course of twenty or thirty exercises on water pipes in which should be discussed all the methods of manufacture and processes of laying cast-iron, wrought-iron lap-welded, steel-riveted and wooden mains, together with a comparison of their relative economies under different conditions in different parts of the country. These lectures, however, would plainly be of such a technical nature that the advantage to the student would be slight; they would give valuable information, but little training.

In all courses in construction and design the practical limit seems to be reached when the exercises are of such a nature as to give more information and little scientific training. The aim of all education, and of engineering education in particular, should be to render the student conscious of his mental power and sure of applying it with scientific accuracy so as to secure economy of construction. Fundamental principles are hence more important than the details of a trade, and all exercises in design should be arranged so that the student may think for himself rather than blindly copy the best practice of the best engineers.

The subject of applied mechanics, which occupies an intermediate place between rational mechanics and the work in design, has been so differentiated that the mechanics of materials is now almost the only topic common to all engineering courses. The strongest line of development has here been in the introduction of testing machines and in the making of commercial tests. This work is of high value, although it may be doubted if the use of one or two large testing machines is as advantageous as that of many smaller ones which are designed especially to illustrate principles. The student of the present day enjoys, however, advantages that were unknown a quarter of a century ago, and the marked progress in applied mechanics from both the scientific and technical point of view is a source of congratulation.

English and modern languages are generally called culture subjects, and it is well known that of all the topics in the engineering course these are the ones in which students have the least interest. The great importance to an engineer of being able to clearly and correctly write his own language can scarcely be overestimated. Further it may be said that no engineer can hope to attain eminence unless he can read German

and French literature. These opinions have long been held, and furthermore it has been recognized that engineering students and graduates are often lacking in that general culture which the world demands as one of the conditions of success. Great improvements have been made in the methods of teaching English and modern languages, and probably still greater ones are yet to result. In the ideal engineering colleges of the future perhaps these subjects will be required for admission, as is now done at least by one institution, but at present they must generally be taught. The main line of improvement to secure better results will be, it seems to me, in partially abandoning the idea of culture and placing the instruction upon a more utilitarian basis. If English be regarded as a means to an end instead of linguistic drill; if the aim of teaching French and German be to read fluently the language of to-day instead of laboriously to decipher the meaning of the poets of centuries ago, true zeal on the part of the student will arise and a truer culture will result.

At the close of the college course the student presents a thesis showing his ability to apply the principles and rules of engineering in the investigation or design of a special problem. The tendency has been strong to abandon subjects which involve mere description or compilation, and to insist upon those that will require the student to exercise his own powers. Thus the value of the work to the student has been greatly increased, and the theses of each class are a source of stimulus to the following ones. Although the view held by some that theses should be monographs setting forth important conclusions of original investigations is one that can not in general be realized, it is a gratification to note that each year a few theses are produced which are sufficiently valuable to warrant immediate publication.

The formation of engineering clubs among students for the discussion of the details of professional work is one of the most important tendencies of recent years. No exercise is so valuable to a student as one entirely originated and performed by himself, and the preparation of a paper which is to be presented to and criticised by his fellows ranks highest of all among such exercises. Recently there has been forced upon my notice a remarkable activity in the three engineering clubs of a certain engineering college, more than fifty papers having been read discussed during the year by a total of about three hundred and fifty students, besides a number of others read before the mathematical club. In meetings of this kind the scientific and economic questions under discussion in the engineering journals receive a detailed attention which the professor in the class room often finds it impossible to give, while the advantage to students in expressing themselves in debate is very great.

Occasional lectures to classes by practicing engineers have been introduced in many institutions during the past decade, and with uniformly good results. In engineering education there is no conflict between theory and practice, and every professor cordially welcomes distinguished engineers to explain their great structures and achievements to his classes. It is an inspiration to students to see and hear those men who have so successfully applied sound science to economic construction, and whose influence has been uniformly to elevate the standard of the profession.

After four years of work the engineering student receives his degree and is ready to commence the actual work of life. What the letters are that designate the degree is a matter of small importance. Moreover, if we examine the lists of the alumni who graduated ten or fifteen years ago, the conviction arises that their particular course of

engineering study has not been an absolute factor in determining their actual lines of engineering work. It is found that graduates in civil engineering are engaged in mining, in machinery and in electricity, and that graduates in other courses are employed upon work in which they received no especial technical instruction. Thus it appears also that the particular course of engineering study is not so important a matter as students and the public generally suppose. In fact, a young man thoroughly grounded in fundamental principles and well trained how to apply them has almost an equal chance for success in all branches of engineering practice.

Looking now over the field of tendency thus briefly outlined it is seen that there has been ever present a powerful impulse towards specialization, to which, indeed, nearly all others have been subordinated. This has demanded a higher standard of admission, great thoroughness in all fundamental subjects, and a rigid adherence to scientific methods. Engineering education has had an active part and healthy growth; it now enjoys the respect and confidence of the public, and its future is sure to be more influential than its past. It is not specialization that has caused its success, but rather the methods which specialization has demanded. Those methods have resulted in imparting to students zeal and fidelity, a love of hard work, a veneration for the truths of science, and a consciousness of being able to attack and overcome difficulties; these elements of character are, indeed, the foundation of success in life.

Looking now forward into the future it is seen that in our efforts for the promotion of engineering education a wide field for work still lies open. The student should enter the engineering college with a broader training and a more mature judgment. The present methods of instruction are to be rendered more thorough and more scientific. In particular the fundamental subjects of mathematics, physics and mechanics are to be given a wider scope, while the languages and the humanities are to be so taught as to furnish that broad, general culture needed by every educated man. In general let it be kept in mind that education is more important than engineering, for the number of men who can follow the active practice of the profession will always be limited. Hence let it be the object of engineering education to influence the world in those elements of character that the true engineer possesses, so that every graduate may enter upon the duties of life with a spirit of zeal and integrity, with a firm reliance upon scientific laws and methods, and with a courage to do his work so as best to conduce to the highest welfare of his race and his country.

Mansfield Merriman.
Lehigh University.

AN OZARK SOIL.

Centrally located on the Ozark Plateau, in the southwestern portion of the State of Missouri, there is a tract of very hilly country, underlain by Lower Carboniferous limestones and noted for its exceedingly stony soil. It comprises a portion of the counties of Stone and Barry, and is bounded on the north and west by the gently undulating plateau country commonly known as the 'crest of the Ozarks;' on the south and southeast by the escarpment of the Lower Carboniferous strata which bounds the broad basin-like valley of White river, and on the northeast by the outcrop of the Ozark Series. This small geographic district is characterized by ridges which are from 200 to 300 feet in height, yet so narrow that often two ridges and two valleys are required to make a mile. It is to the soil which covers these steep narrow ridges that I wish to call attention.

Our district being without the limits of the glaciated areas possesses a soil which has been in process of formation during many periods. It has never been disturbed by either marine or lacustrine agencies, and consequently, is but the residual material accumulated on the surface of the limestone rock after the decay of higher strata. These strata consisted of the coarsely crystalline, crinoidal Burlington limestone and abounded in layers of white chert.

The surface stratum of the soil which we are now considering is a layer of angular white chert gravel. The pieces vary in size from very small to a cubic foot, but sizes of a cubic inch to ten cubic inches predominate. Where the ridges are narrow the surface is so completely covered with this broken chert that the true soil cannot be seen, and in the spring, when the brown oak leaves and dried prairie grass are burned off, the hills look like hugh piles of broken rock. On the steeper hillsides the chert layer, which is here a true talus, is often several feet in thickness, and no attempt is made to reach the underlying soil for the purpose of cultivation. But on the flat-topped ridges, the plow passes under the superficial chert into a rich black soil, which is six to eight inches in thickness and remarkably fertile. This soil layer is nearly free from large fragments of chert, although very small particles abound and aid in giving the soil a very loose texture. The black color is, of course, derived from the decay of vegetation, and the carbonaceous matter accumulates more rapidly where the overlying chert layer is thickest. In fact, the existence of a black soil in this latitude is probably largely due to the presence of the chert.

Several years ago a 'cyclone,' in passing across the hill tops in the vicinity of Rancho Springs, in Stone county, prostrated the timber in narrow belts. The fallen oaks have upturned the soil, producing fine sections through it. Under the dark soil layer we find a light yellow clay, at first nearly free from chert, but which, at the depth of two feet, contains such a large percentage of large chert fragments that it requires the use of the pick in excavating it. This yellow sub-soil is a stiff clay and, when puddled with water and plastered into the 'chinking' between the logs of the simple country houses, makes an excellent substitute for mortar. When plowed into, rained upon and dried, it hardens on the surface as though frozen, so that to walk over a plowed field when it is in this condition makes no impression on it. Yet it contains the elements of fertility and in time will weather into a good soil.

The yellow subsoil clay grades imperceptibly downward into a bed of closely packed but invariably fragmental chert. At three feet from the surface of the soil, less than 10% of the material is clay, occupying the narrow crevices among the chert. At this depth also the yellow clay changes rather abruptly to a similar fine-grained stiff clay of a bright brick-red color. From here to the surface of the limestone rock, which may be 10 or 20 feet from the surface of the soil, the mass is composed almost exclusively of the fragmental chert. What clay there may be among it is always of the bright red variety.

Now it is to the characteristic feature of the subsoil clay, viz., its color, that I wish to call special attention. This, as we have just seen, differentiates naturally into an upper yellow variety and a lower red variety. The line of demarcation between them is not sharp, and bears no definite relation to the main body of the chert. For when the ridges are broad, and the subsoil clay over the chert bed thickens, the surface of the clay rises into the subsoil stratum, leaving quite a thick bed of not very stony red clay over the main body of the chert. In short, as the line of demarcation persists in

following a given depth under the surface, which is about three feet, it is evident that the difference in color is due to a modification of the red clay by some action either atmospheric or aqueous. Now, red clay is the natural residual product of the decaying limestone. Red is, also, the color most generally represented in the mud of the caves. Indeed, there is no macroscopic difference between the red cave earth and the clay on the limestone rocks outside. They are due to the same general cause and constitute the same formation. But the upper three feet of the residual clay on the ridges has been converted into a yellow clay. The same effect has been observed and recorded, by numerous writers, in other unglaciated districts, but in this it is perhaps more prominent than in others. The cause appears to have been not the action of the atmosphere, which is incapable of destroying and removing the red oxide of iron, but the solution and removal of a large part of the iron salt, by percolating water containing acids generated by the decay of the vegetable matter contained in and on the soil.

The writer, believing that certain colors are, to some extent, characteristic of the products of certain periods and certain climates, wishes to propound the following questions:

1. If a residual clay were to form, in the absence of vegetation, at the present time in the Ozarks, would it be yellow immediately or would it first pass through a stage of red color?

2. Did the pre-glacial residual material, in certain districts of the upper Mississippi basin, as, for instance, over the Galena and Niagara limestones of northwestern Illinois, have a yellow stratum over the ordinary red, as in the present subsoil of Stone county, Missouri? (The remains of the pre-glacial residua yet seen by the writer in northwestern Illinois indicate only a red subsoil.)

3. Is it not possible, indeed probable, that the red clay in southwest Missouri represents some ancient period, while the modification of its upper three feet into a yellow clay is peculiarly the result of a more recent period?

The writer does not intend to answer these questions, but in conclusion will state one fact, which bears strongly on the last and may be found to be a key to its solution. In some long-past period the streams in Stone county laid down a flood-plain of of gravelly clay and silt of a prevailingly bright red color. Obviously, the material came from the soil and subsoil clay of the surrounding ridges. During a later period the same streams laid down a flood-plain of a light brown and yellow color. Obviously, the material came from practically the same position as during the earlier period. For an explanation of the strong contrast between the two fluvial formations we must look to the surface portion of the residua on the ridges. If we read the evidence aright it indicates that, subsequent to the formation of the first river deposit, a change of climate converted the previously red surface portion of the residual clay into a yellow clay, before the advent of the period during which the later formation was deposited.

Oscar H. Hershey.

Freeport, Ill.

CURRENT NOTES ON ANTHROPOLOGY.

SOCIAL ORGANIZATION OF THE INCAN GOVERNMENT.

Under the title, 'Die sociale Verfassung des Inkareichs' (Dietz, Stuttgart), Dr. Heinrich Cunow, already known by an able treatise on the Australian aborigines, presents an analysis of the government and sociology of the Peruvians before the advent of the Spaniards. It is written from a careful comparison of the best early authorities and in the spirit of modern sociological science. The subject, therefore, is presented in a widely different light from that offered in Prescott's History. The

foundation of the Peruvian government was an agrarian communism derived from the rights of the primitive gentes, very much as was the case not only in other parts of America, but, as the author observes, among the ancient Aryans as well. This explanation he develops in a highly satisfactory manner.

The claim, however, which Mr. Cunow puts forward in his preface, that he is the first to make these facts clear, is, doubtless unwittingly, unjust to a worthy American student, Dr. Gustav Brühl, who in his learned volume, 'Die Culturvölker Alt-Amerikas,' Chap. XVII. (Cincinnati, 1887), traces with entire clearness the Peruvian organization to the same source as does Cunow. It is to be hoped that in a future edition the latter will make proper acknowledgment of this.

THE INTERNATIONAL CONGRESS OF AMERICANISTS.

Mr. E. de Olivarria y Ferrari has issued at Mexico the 'Cronica del Undesimo Congreso Internacional de Americanistas' (pp. 183), giving a narrative of the proceedings of the Congress, its meetings and excursions (not abstracts of papers). The outlines were reported to Science at the time by Mr. Halsted. The present volume proves still further how courteous and kindly was the reception accorded to the Congress by the authorities and citizens of Mexico.

That meeting, however, was not a regular, but an extra session. The Congress meets only once in two years, and at the last regular meeting, in Stockholm, 1894, it was agreed to convene next in Holland, probably at the Hague. This is still the intention, and the last number of the 'Internationales Archiv für Ethnographie' contains an announcement to that effect. The precise date will be determined later. The volume of proceedings at Stockholm has not yet been issued. The Compte-rendus of the Congress, now numbering many volumes, the first of which was published in 1875, contain numerous articles of value to the student of the archæology and languages of America.

WORD-COUPLING LANGUAGES.

Sometimes a single linguistic procedure serves as a valuable trait by which to group linguistic stocks and measure their relative development. Such is the plan of uniting words one to another, so as to form compounds. This has been studied by several writers, and lately by Dr. H. C. Müller, of Leyden, in a monograph, 'Beiträge zur Lehre der Wortzusammensetzung' (pp. 59). While mainly devoted to the Aryan group, he has the breadth of mind, rare among Aryan specialists, to remember that all tongues are not built on Aryan models, and therefore calls under consideration the Ural-Altaic, Australian, and even, *mirabile dictu*, the American languages, for purposes of comparison. In this particular field the last mentioned offer peculiarly abundant topics of study in their synthetic and incorporative character, to which the author alludes, but perceives that the field is too vast to be surveyed in a few pages.

In some groups of tongues, as the Sinitic, word-coupling cannot be said to exist in the sense of the *dvandva* of the Sanskrit grammarians; under certain restrictions, its presence and development lend flexibility, accuracy and poetic power to a tongue, and thus serves as a criterion of linguistic evolution. This and other suggestive thoughts will be found in the essay.

D. G. Brinton.

University of Pennsylvania.

SCIENTIFIC NOTES AND NEWS.

MEMBERSHIP OF THE INTERNATIONAL CONGRESS OF APPLIED CHEMISTRY.

An editorial in the *London Saturday Review*, August 1, 1896, makes the following comment

upon the address of M. Berthelot, as President of the International Congress of Applied Chemistry, recently in session in Paris. It is of interest in this country, not only as showing the present attitude of thinking Englishmen in regard to the encouragement of research, but because the reproaches which the English editor showers upon his own country are at least equally applicable in the United States, perhaps more so:

"He was addressing the generals of this new army of science, who, in the rivalries of their nations, count for more than hosts of armed men. England, to defend her vast and scattered interests, attempts to keep her navy equal to the combined navies of any two foreign powers. How in this scientific review did she compare? The figures are so startling and so omnious that we give them all: from Austria, 157; from Germany, 102; from Belgium, 53; from Russia, 37; from Peru, 35; from Portugal, 25; from Brazil, 25; from Mauritius, 24; from Holland, 23; from the United States, 20; from Spain, 19; from Switzerland, 13; from Egypt, 12; from Italy, 10; from England and from Greece, 8; from Roumania, 7; from Cuba, Mexico and the Argentine, 4; from Denmark and Turkey, 1. Repeat it, ponder it! From England, 8; from Austria, 157; from Germany, 102. We will warrant that the Rev. Dr. Lunn got more Englishmen to attend his Swiss Conference on Arbitration! The worst of it is that we have little doubt but that the numbers represent fairly the relative interests in technical chemistry in the different countries, especially if allowance be made for convenience of access to Paris, the place of conference. For the present we cannot enter at length into the causes and possible remedies for this national folly. But we may point out that vast sums are annually wasted on chemistry in England. The Science and Art authorities at South Kensington, and the Technical Instruction authorities of the County Councils, spend largely upon chemical subjects. But, for the most part, the money is spent upon teaching of chemistry, not upon chemical research. It may be a valuable addition to national character that a large number of children be taught the elements of water and the composition of coal gas. But it is an indisputable fact that ninety per cent. of these children do not proceed beyond the luxury of superfluous elementary knowledge; and that of the remaining ten, at least nine become themselves elementary teachers. Teaching is a trade in England; research is not; and, until the endowment of research is recognized as a million times more important than the diffusion of cheap knowledge, England will continue on the downward path."

'SQUIRTING' IRON AND STEEL AND OTHER METALS.

ONE of the most remarkable and unexpected developments in the recent progress of manufactures of metal is described by Mr. Nursey in a paper recently presented, at the Spring Meeting, to the Iron and Steel Institute of Great Britain. This is a process of 'squirting' bars of all the metals in a manner similar in principle to the old and familiar process of manufacture of lead pipe. It is the invention of Mr. Alexander Dick, long known as a practical metallurgist, and especially in the work of introduction of various valuable alloys.

Mr. Dick has discovered a way to make possible the production of all sections of metal bars from the simple round wire to the most complex designs, such as are quite impossible to roll successfully, by raising the metal to be thus formed to a high temperature, and thus to reduce it to the plastic state and then forcing it from a reservoir through properly formed dies under hydraulic pressure. His claim is that it is commercially practicable to form bars of all such sections by 'extrusion under pressure at high temperatures.' The temperature usually adopted by him is approximately 1000° F.

After a long and costly series of experiments, the following system of construction of the apparatus has been found to meet the requirements of the case successfully: A series of concentric cylinders of tungsten steel are placed one within another, separated by an intermediate space of about $\frac{3}{4}$ of an inch; which space is filled with compressed non-conducting material. This 'container' is mounted on trunnions and fitted with a worm-gear arrangement for swinging it in the vertical plane, like a Bessemer Converter. The die plates are made of tung-

sten steel, and their orifices are given the form of the proposed section of the bar to be made. They are carried in a holder which permits their convenient introduction and removal for substitution of one form for another. Before the operation begins, the 'container' is set vertically to receive the charge, and the dies and holder are heated, also, to prevent chilling. Once charged, the container is turned into the horizontal position, and the plunger of a hydraulic press, working under about two tons per square inch pressure, is forced into the container, driving the plastic metal out through the die, from which the bar issues of the desired sectional shape.

The preliminary heating is effected by gas-burners, and the operation of the apparatus keeps up its temperature to the required point until its working hours are over. The apparatus in use at the works of Mr. Dick, the Delta Metal Co., London, has a cylinder of about two feet external diameter and an inner liner five or six inches. The product is about fifty charges per day, and the cost of operation is claimed to be small, the wages of two men and a boy. The metal so produced is claimed to have greatly increased strength, as compared with that made by simple casting, in the usual manner, precisely as 'Whitworth steel' is improved by pressure. Common yellow brass gains about 24 per cent. in tenacity. 'Delta metal' bars thus made are reported to have a tenacity of 48 tons per square inch as against its former strength, 32 tons, and to exhibit a ductility of 32.5 per cent. as against 20 per cent.

Only the copper-tin-zinc alloys and similar metals have, as yet, been treated ; but the inventor proposes ultimately to employ the process in the manufacture of iron and steel bars of difficult sections.

THE SANITARY VALUE OF SUNLIGHT.

AT the Annual Congress of the British Institute of Public Health, which was held at Glasgow, from July 23d to July 29th, Professor Ramsay, of University College, London, in his address as President of the Chemistry and Engineering Section, dwelt on the sanitary value of sunlight. According to the report in the *British Medical Journal*, he said that the most common evidence of the activity of the violet and ultra-violet rays is sunburn, which is probably due to the effort of the surface cells to protect themselves against these rays by secreting a pigment which can absorb them, and the peeling which accompanies severe sunburn is merely the shedding of such dead cells as have been unable sufficiently to protect themselves. The Röntgen rays are particularly apt to cause the worst kind of sunburn, in one case causing the finger nails of a hand which had been repeatedly subjected to them to come off. Professor Ramsay drew attention to the well-known researches of Professor Marshall Ward, in which he found the violet and ultra-violet rays of the sun, or even of electric light, to be capable of disinfecting the bacilli of typhoid and anthrax. The same subject has now been investigated from the chemical side by Dr. Arthur Richardson. Dr. Richardson determined the circumstances which caused the fading of certain water colors when exposed to light, and examined the action of light on carbon compounds and urine. He found that the effective agent in bringing about the changes which he observed is peroxide of hydrogen. When certain organic materials, such as carbolic acid, and some alcohols, or oxalic acid, are exposed in a damp state to sunlight, hydrogen peroxide is produced. Two quantities of fresh urine were tested for hydrogen peroxide ; none was found. One was then exposed for six days to sunlight; the other was kept in the shade. The exposed sample was clear and was found to contain peroxide, while the portion kept in darkness swarmed with bacteria, had grown foul and contained no peroxide. Even after 23 days' exposure to sunlight the one showed no putrefactive change, while the other was entirely putrefied. Similar experiment was made where one sample was shaded with ruby glass, decomposition and absence of peroxide ensuing, while the portion exposed to sunshine was quite unaltered in appearance. Some of the sunned samples, after they had developed a considerable quantity of peroxide, were kept in the shade, but immediately developed fungoid growths, and the peroxide disappeared. Further experiment showed that the presence of oxygen was necessary for

the formation of peroxide in urine. From these results, coupled with those of Dr. Edward Frankland on the development of bacteria at various depths below water, Professor Ramsay regards it as proved that the action of violet and ultra-violet light on organic matter may lead in many cases to the formation of peroxide of hydrogen; that peroxide passes on a portion of its oxygen to the organic matter, thus becoming water and destroying or changing the organic matter; that such changes are destructive to the minute organisms contained in rivers, and generally to animal life, unless the organism is capable of secreting some pigment which excludes violet and ultra-violet light; and that certainly typhoid and anthrax, and probably also other zymotic disease, would be prevented if it were possible to subject the source of infection to sunlight in the presence of moisture.

GENERAL.

JOSEPH DWIGHT WHITNEY, professor of geology in Harvard University, died at noon on August 19th, aged 76 years.

ALBERT NELSON PRENTISS, who since the foundation of Cornell University, in 1868, had occupied the chair of botany, arboriculture and horticulture, died at Ithaca, on August 14th.

FOLLOWING closely on the death of Sir Joseph Prestwich comes the news of the death of Alexander Henry Green, also professor of geology at Oxford. Prof. Green was a student of Cambridge and a fellow of Caius College. He was for many years attached to the geological survey of England and Wales, and later became professor at Leeds, being appointed to the professorship of Oxford in 1888. He died on August 20th, at the age of sixty-four.

THE most recent advices indicate that not only in the north of Japan, but also in Norway and Russia, observations of the recent solar eclipse were made impossible by clouds.

A FIRE broke out on August 18th in the building of the Industrial Exhibition in Montpellier, France, which not only cost considerable injury to the exhibits, but also spread to the university buildings, the damage to the latter being estimated at 600,000 francs.

ACCORDING to the provisions of the will of the late George W. Wales, the Boston Museum of Fine Arts, after the death of his widow, will get $30,000, besides his collection of pottery and glass, and his books on pottery, engraving, glass, lace, painting, architecture and fine arts in general.

DR. A. BALDACCI has undertaken, during the present year, a botanical investigation of northern Epirus, especially the district of Konitza.

DR. NANSEN has contributed to the London *Chronicle* a detailed account of his adventurous exploration, which has been cabled to this country and read by everyone in the daily papers. He states that during the drift of the Fram northward he made careful series of scientific observations, meteorological, magnetic, astronomical and biological, soundings, deep-sea temperatures, examinations for the salinity of the sea water, etc. The sea was not more than ninety fathoms deep south of 79 degrees north, where the depth suddenly increased and was from 1,600 to 1,900 fathoms north of that latitude. This will necessarily upset all previous theories based on a shallow polar basin. The sea bottom was remarbably devoid of organic matter.

DR. HERMANN KRUTZSCH, lately professor of physics and meteorology at the University in Tharandt, died on July 28th, at the age of 77. We also note the death of Dr. L. C. Wiener, professor of mathematics in the High School of Karlsruhe, at the age of 70.

MR. WILFRED WARD, who was Huxley's neighbor at Eastbourne during the latter years of his life, has contributed to *The Nineteenth Century* an interesting account of conversations with Huxley. He is reported to have said: "One thing which weighs with me against pessimism, and tells for a benevolent Author of the universe, is my enjoyment of scenery and music. I do not see how they can have helped in the struggle for existence. They are gratuitous gifts."

THE Editor of *The Astronomical Journal* announces that a few copies of the *Astronomische Nachrichten*, Vols. 100–140 inclusive, forty-one volumes in all, may be obtained from him for $75.00, which is much less than the regular price.

PROF. A. A. TICHOMIROW has been appointed director of the zoological museum of the University of Moscow, in place of the late Prof. Bogdanov.

THE biological station at Plön was closed during the month of July, and has during this period been thoroughly renovated. It is open during August, a month especially favorable for a study of the fauna of the lake. The charge is 10 M. a week for the use of a table.

THE committee appointed by the Kazan Physico, Mathematical Society to collect funds for the Lobatchefsky memorial have received 9072 roubles ($7165). *Nature* states that a circular issued by Prof. Vassilief contains the information that the fund has been utilized in the following manner: A capital sum of 6000 roubles has been used to found a prize of 500 roubles to be awarded every three years for a geometrical work, and especially one on non-Euclidian geometry, printed in Russian, French, German, English, Italian or Latin. The first prize will be awarded on November 3, 1897 (the centenary of Lobatchefsky's birth took place on November 3, 1893), and mathematicians competing for it must send in their works not later than November 3d (October 22d). The sum remaining after the foundation of this prize has been devoted to the erection of a bust of Lobatchefsky, in front of Kazan University. The bust will be inaugurated on September 13th of this year, and it is hoped that as many foreign men of science as are able will be present to witness the ceremony.

THERE will be held at Turin in 1898 a National Exposition at which special arrangements will be made for meetings on medicine and hygiene.

DR. GEO. BRUCE HALSTED, professor of mathematics in the University of Texas, is spending the summer in Austro-Hungary and Russia, where he is engaged in mathematical research. His address is Kazan, Russia.

THE British Commissioners for the Exhibition of 1851 have made twenty appointments to science research scholarships, for the year 1896, on the recommendation of the authorities of the universities and colleges in which this right is vested. The scholarships are of the value of £150 a year, and are ordinarily tenable for two years in any institution approved by the Commissioners. The scholars are to devote themselves exclusively to study and research in some branch of science, the extension of which is important to the industries of the country.

THE University of the Pacific, at its last commencement, conferred the honorary degree of Doctor of Science upon Prof. Edward S. Holden, Director of the Lick Observatory.

HENRY C. FORD, President of the Pennsylvania State Fish Commission, died at Philadelphia on the night of August 17th, at the age of sixty.

Since the outbreak of cholera in Egypt this year to August 15th there have been 14,755 deaths.

THE agricultural experiment station of the University of Wyoming has issued a first report on the flora of Wyoming by Prof. Aven Nelson, botanist of the station. There are enumerated from the material in the herbarium 1,118 species and varieties of phanerogams representing 393 genera, and 170 more have been reported by other observers. Though the northeast and southwest floras are quite distinct from each other and from those portions of the State which have been the most carefully examined, 1,295 species and varieties have thus far been reported from the State, as compared with 1,460 from Nebraska and 1309 from West Virginia, two of the States that have been most carefully worked over.

THE 119th part of the *Flora Brasiliensis*, containing Orchidaceæ III., by A. Cogniaux, has been published in Leipzig. The cost of this extensive work, which was begun in 1840, now amounts to nearly $1,000.

THE last number of the *Transactions of the American Institute of Electrical Engineers* contains a report on standards of light by a subcommittee of the Institute consisting of Edward L. Nichols, Clayton H. Sharp and Charles P. Mathews. The committee concludes that of all standards thus far used candles are the least reliable. It seems likely that many of the difficulties that are unavoidable with flame standards may be overcome by the adoption of a standard consisting of some surface electrically

heated to a standard temperature. The definition of the degree of incandescence of such a surface appears at the present almost insuperable, but the committee is at work upon a method for the measurement of the temperatures of incandescent carbon, which may lead to results looking towards the solution of the problem. It also has in progress experiments looking to the production of a light standard in which not only the burning material, but also the atmosphere, shall be of known and definite chemical composition.

PROF. E. B. TITCHENER, of Cornell University, will translate into English Wundt's *Physiologische Psychologie* and, in coöperation with Mr. W. B. Pillsbury, Külpe's *Einleitung in der Philosophie*. Miss Julia H. Gulliver, of Rockford College, will translate Wundt's *Ethik*. WUNDT'S *Lectures on Human and Animal Psychology* and Külpe's *Outlines of Psychology* have already been translated into English by Prof. Titchener, the former in coöperation with Prof. Creighton, and his *Introduction to Psychology* is being translated by Dr. Judd. Consequently, after a too long delay, we shall have adequate English versions of Wundt's contributions to psychology, including those of one of his most prominent pupils, Prof. Külpe.

THE Chicago Academy of Sciences gave, from July 15th to August 15th, a free course of lectures by twenty-five different lecturers, the subjects included being Anatomy, Climatology, Optics, Geology, Astronomy, Physics of Electricity, Botany, Zoology, Entomology, Comparative Anatomy, Mental Science, Biology, Physiology, Malacology, Physical Geography, Surgical Anatomy, Physics of Optics, Bacteriology, Ornithology, Scientific Nursing, Latin, German, Anthropology, Chemistry, Surgical Philosophy, Medical Chemistry, Hygiene and Meteorology. Meteorology was demonstrated at the Auditorium Tower every Saturday afternoon from 2 to 4 o'clock by Prof. E. B. Garriott.

THERE has been established in Berlin a People's Society for Natural Science, the chief object of which is to offer scientific lectures that will be interesting to those having no technical knowledge of the subject. The first lecture before the Society was given by Dr. Förster, Director of the Royal Observatory, and was entitled 'Conditions and beginnings of life on the earth.'

A CASE reported in *The British Medical Journal* may be of interest to American as well as English municipal authorities. The executors of the late Mr. T. H. Smith, of Moseley, near Birmingham, claimed, on behalf of his widow and six children, the sum of £10,000 damages from the King's Norton Rural District Council for negligence, which, it was alleged, caused the death of Mr. Smith from typhoid fever. It was stated that a ventilating shaft communicating with a chimney in the house of the deceased allowed sewer gas to be conveyed into the rooms. In 1891 the Council undertook to disconnect the shaft, but after Mr. Smith's death inquiry was made, with the result, it was alleged that a defect was found to exist which, still permitted the escape of sewer gas into the house. The jury gave a verdict for the plaintiff, with £3,500 damages.

WE learn from the New York *Evening Post* that, beginning with August 28th, the usual series of horticultural schools under the direction of the professors at Cornell will be conducted throughout western New York. Twenty-six schools have been definitely arranged for, to be in the charge of three distinct sets of teachers, having at their heads respectively Prof. L. H. Bailey, who travelled 15,000 miles back and forth through the State in carrying out this work last year; George T. Powell, of Ghent, and Prof. E. G. Lodeman.

UNIVERSITY AND EDUCATIONAL NEWS.

THE University of Pennsylvania will proceed at once with the erection of a new building for the department of dentistry, to cost $120,000.

GROUND has been broken for the new library at Princeton University. The building is to be 200 x 180 feet in ground measurements, and will be four stories high. The estimated cost is $598.000.

THE London University Bill, which it was hoped to pass through Parliament before its adjournment, has been abandoned, owing to the

opposition of a few members of the House of Commons who wish to introduce a Church of England 'Test' for the professorships in Kings College.

COL. W. C. BRECKINRIDGE, of San Antonio, has given $30,000 to the University of Texas to be used for a dormitory and confectory for the women students of the medical department.

DR. A. C. ABBOTT has been appointed to succeed Dr. Billings, who has resigned the chair of hygiene in the University of Pennsylvania.

DR. EDWIN F. NORTHRUP, of Syracuse, N. Y., has been elected associate professor of physics in the University of Texas. Dr. Northrup is a graduate of Amherst College, was later a graduate student at Cornell University, and a fellow for two years at Johns Hopkins University.

MR. B. M. DUGGAR has been appointed assistant in cryptogamic botany in Cornell University.

PROF. THOMPSON has resigned from the chair of ophthalmology in Jefferson Medical College and will be succeeded by Dr. de Schweinitz.

DR. BURNEY YEO has been appointed by the Council of King's College professor of the principles and practice of medicine in succession to Dr. Lionel Beale, and Dr. John Curnow has been appointed to the chair of clinical medicine in succession to the late Sir George Johnson.

DR. C. v. EHRENFELS, of Munich, has been appointed assistant professor of philosophy in the University of Prague, and Prof. R. Anschütz, of Bonn, has been appointed acting director of the Chemical Institute of the University.

DISCUSSION AND CORRESPONDENCE.

A PROTEST AGAINST QUADRINOMIALISM.

IN the present days when systematists are continually confronted with puzzles in nomenclature, which owe their origin mainly to the inadequate descriptions and careless methods of the older naturalists, it seems of the greatest importance for us to consider carefully any new practices that may be proposed by writers of today, and to call attention to their good or bad points before they are adopted by others.

A case in point will be found in papers by Dr. C. Hart Merriam, in 'North American Fauna,' Nos. 10 and 11. This is practically the proposition to introduce 'quadrinomials' into our nomenclature. Trinomialism, the use of 'subspecies,' has of late years become almost universal among zoologists in this country, and its advantages are well known. Dr. Merriam has always been a strong advocate of trinomialism, but in the papers just referred to he goes a step further and describes 'subspecies' of 'subspecies,' which is practically quadrinomialism, though he so writes the names as to use only three words.

For instance, *Blarina brevicauda*, is the common short-tailed shrew of the northeastern States. In the Southern States it merges into a smaller subspecies known as *Blarina brevicauda carolinensis*. In tropical Florida still another subspecies is found which Dr. Merriam names *peninsulæ*. This, according to our established usage would stand as *Blarina brevicauda peninsulæ*, but Dr. Merriam writes it *Blarina carolinensis peninsulæ* (N. A. Fauna, No. 10, p. 14).

It will be noticed that *carolinensis* is thus treated as the 'species' of which *peninsulæ* is made a subspecies, while on the preceding page *carolinensis* is itself treated as a subspecies of *brevicauda*.

In other words, the author prefers to unite in the trinomial name the two forms which are geographically contiguous and to omit the fourth name, *brevicauda*, for the sake of brevity. This is of course nothing more nor less than quadrinomialism.

There is no doubt but that quadrinomials or any form of polynomials may be employed under the same rules that now govern us in the use of binomials and trinomials, but the question arises, are they desirable or useful? Decidedly not. Trinomials serve a useful purpose in the designation of geographical races, which while quite distinct in their extremes are connected by a perfect series of intergrades. No other satisfactory method of designation has ever been suggested for these. But here let us draw the line. We cannot express the whole relationship of a species in its name, and if we could the name would become useless as such; and when it comes to dropping out a portion of it we only tend to confusion. Do we not lose

just as much by omitting *brevicauda* in the instance quoted as by inserting *carolinensis*?

Moreover the adoption of such a practice will necessitate the rearrangement of most of our existing trinomial nomenclature, and in such cases as the Song Sparrows or Horned Larks among our birds it will be no small matter to decide which of the numerous subspecies shall be relegated to 'sub-subspecies' and in which instances the species name shall be omitted.

Furthermore, is not an author who uses quadrinomials, expressed or implied, placing himself in the same category with Brisson and other post-Linnean authors who were more or less polynomial? And when we ignore their works entirely, what right have we to recognize more recent writers who are not consistently binomial?

In conclusion it seems to me a matter of serious regret, when the A. O. U. Code of Nomenclature has practically become the standard for American zoologists and botanists, to see a member of the A. O. U. Committee on Nomenclature breaking away from the Code and proposing such innovations as the above. Is not such individual action directly opposed to the ultimate stability of our nomenclature?

WITMER STONE.

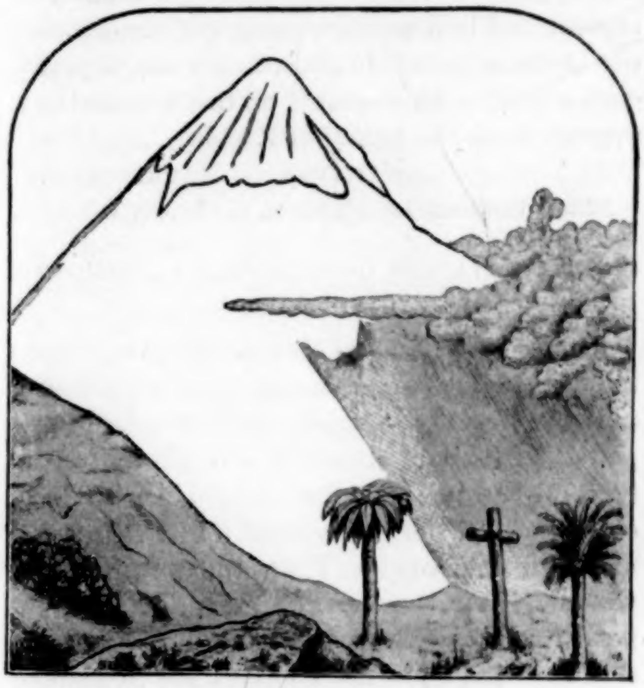
ACADEMY OF NATURAL SCIENCES,
PHILADELPHIA, August 3, 1896.

IMPOSSIBLE VOLCANOES.

TO THE EDITOR OF SCIENCE: I wish to echo the protest expressed by J. Paul Goode in a recent issue of SCIENCE, against the illustrations of impossible icebergs, with which our text-books are filled, and ask that impossible volcanoes be put in the same category.

The picture which has done service in geographies for many years as a representation of Popocatapetl is about as severe a libel on a respectable volcano as one could well imagine. A tall cross, such as no traveller in Mexico ever saw, and luxuriant palms such as never grow at the altitude from which Popocatapetl can be seen, make up a tropical foreground beyond which a symmetrical, snow-capped cone with a slope of from 40° to 50° rises to an impossible height and extends to an impossible magnitude.

All this is untrue, and it would seem, considering the number of excellent photographs of the volcano extant, unnecessary. Besides, it tends to perpetuate a common misconception as to the slopes and heights of mountains which it is time to correct. Many of the pictures of mountains appear rather to record the feelings of the artist after he has climbed to their summit than to represent their actual profile.



It ought to be generally understood that the average slope of a mountain of any kind can rarely be more than 35° and is usually much less. During a recent visit to Popocatapetl, I measured its slope from several points of view, and found it never more than 30°. In making a sketch of the volcano, however, I found that I labored under the optical delusion which leads one to exaggerate the steepness of mountain slopes, and which probably accounts for their usual faulty representation. The slope as I represented it on the paper, with what I thought to be a fair degree of accuracy, proved on holding the paper between my eyes and the volcano to be far too steep. It was only after several trials that I could give it the requisite flatness.

On relief maps vertical exaggeration is excusable and without doubt necessary, but it can hardly be said to be in text-book illustrations. Natural scenery is sufficiently imposing not to need to be made attractive by exaggeration, while correct illustrations strengthen the pupil's confidence in the truth of what he is taught.

I append a view of Popocatapetl as it is represented in a modern geography in common use in our schools, and, for comparison, a profile drawn from a photograph of the volcano as it appears from the valley of Puebla.

OLIVER C. FARRINGTON.
FIELD COLUMBIAN MUSEUM, CHICAGO, ILL.

ON THE NOTATION OF TERRESTRIAL MAGNETIC QUANTITIES.

AT the International Meteorological Congress to be held in Paris, a number of questions of special interest to magneticians have been proposed for discussion, among which is the following: The same notation should generally be employed, H for horizontal force, X for the northern component, Y for the western component, Z for the vertical force, and V for the potential. As the need of some uniform notation has been made apparent to me in connec tion with the journal *Terrestrial Magnetism*, I have been paying this matter some attention with the view of obtaining a concise and logical system for adoption in this journal.

The principle upon which I proceed is to take the first letter of a word designating a particular quantity, if at the same time it conforms with typographic requirements, such, for example, as declination, which is common to several languages. In this way I have thus far obtained the following: D for declination, I for inclination, H for horizontal component of force, V for vertical component, F for total force.

Upon examination it will be found that these letters stand for words derived in almost all cases originally from the Greek and Latin languages and, with but insignificant variations in spelling, common to several of the main modern languages. The Germans will be asked to yield a point with regard to F,* but this, as will be seen below, will be made up to them in the adoption of G for magnetic potential. V, taken from the Latin *vis* or I from *intensitas*, or D from the Greek word *δύναμις*, would not do for force, as they are already taken. Nor would T from *totus* or P from *πᾶς* answer, since the former is frequently used for time of vibration, and so in fact is the letter P, which stands besides for the first deflection coefficient. As I hope to be able to find satisfactory notation for all the principal magnetic quantities, I am keeping this matter constantly in mind in adopting any particular letter. The English and French have *force*, and I have, therefore, adopted F for total force. As it is frequently the custom to designate angular quantities by Greek letters, I should have preferred, had it been possible, to adopt δ and ι instead of D and I, but the Greek ι is a very unsatisfactory letter from a typographical standpoint. Moreover, if found desirable later on, the small letters d and i or δ and ι can be reserved for the variations on the mean of day and on the mean of year respectively.

I think it very much to be deplored if Z, as above proposed, be universally adopted to designate the vertical force. It should not be forgotten that the Gaussian mode of resolving the magnetic force into northerly component (X), westerly component (Y) and vertical component (Z) applies to a *local* system of coordinates, not to a fixed system, as the layman might naturally suppose, a fact which is even apparently forgotten at times by magneticians. The mean values of these components for a complete circuit of the earth along a parallel of latitude can, in consequence, no more be *physically* interpreted than the mean H, for example. I am therefore opposed to adopting a letter for the vertical force which in no way gives evidence of the exact quantity for which it stands. V, on the other hand, is logically connected with H and at the same time implies that the direction of the quantity that it symbolizes is *local*, the direction of the vertical or plumb line varying from point to point.

For the same reasons I am not in favor of adopting X for northerly component and Y for westerly component. Let authors choose this method of notation if they prefer it, but in a

*The initial letter of the German word *Kraft* is frequently used to designate the moment of inertia and hence will not answer for force.

system suggested for universal adoption it would seem to me that N and W would more satisfactorily meet the requirements, clearly indicating to the eye as they do the local character of the system of coördinates employed.

As a letter to designate the earth's magnetic potential, I believe none more fitting could be adopted than G after Gauss, the author of this function. Gauss himself used V, but this letter is not sufficiently characteristic; it is used to designate many other functions in mathematical physics; and there would, moreover, be a conflict in our system, since V seems the most logical letter to designate the vertical force.

L. A. BAUER.

LINDEN, MD., August 10, 1896.

SCIENTIFIC LITERATURE.

Memoirs of Frederick A. P. Barnard, D. D., LL. D., L. H. D., D. C. L., Tenth President of Columbia College. By JOHN FULTON. Columbia University Press. Macmillan & Co. 1896.

When a person has been for nearly sixty years deeply interested in the problems of education, and has himself contributed largely to their solution, his biography necessarily reads like the history of the progress of this science during that period. The life under review is no exception, and indeed his lively reminiscences of his own early school days carry the beginning of our period back to the time when our century was scarcely a baker's dozen years old.

Born May 5, 1809, at Sheffield, Mass., of old Puritan stock, Frederick Augustus Porter Bernard was a thorough New Englander. He has given a very vivid description of the isolation of the little village among the hills and its peculiar institutions, especially the 'meeting house' and all its associations. He says of this early period of his life, which he afterwards came to consider all important in the education of a child: "I believe that if there is anything good in me it must be owing to that loving maternal solicitude which gently swayed me toward the right, at a time when the bending of the twig sufficed to give its permanent inclination to the full grown tree." Soon after he could walk he was sent to the village school, and at four attended a 'grammar school.' At six he commenced the humanities with the village parson and was an interested reader of Shakespeare's comedies; with his mother he made the acquaintance of Cowper, Goldsmith, Addison, Burke and others. At the same time his ingenuity produced kites, windmills, waterwheels and the like, which were the objects of the envious admiration of his playmates. At the age of nine he went away to the Saratoga Academy, where along with much classics he learned the printer's trade, an incident which undoubtedly was the beginning of that interest in journalism which resulted later in so much editorial work. When only twelve he was sent to the Stockbridge Academy to prepare for Yale, where he entered three years later (1824), the youngest member of his class. One will be amply repaid for reading his lively and often amusing accounts of his life at the preparatory school, and especially his description of contemporary life at Yale. Graduated second in his class in 1828, he was appointed teacher in the Hartford Grammar School. These two years of life in Hartford prior to his appointment at Yale were full of new experiences and ventures, especially as an author and an editor, and at this early date he evinced that liking and aptness for newspaper controversy that stood him in such good stead in his after life.

When Barnard was appointed to teach at Yale it had been the custom for each tutor to take his share of the entering class and teach them all the branches during their first three years. As an undergraduate he had seen the weakness of this method, and his first act at Yale was to persuade the faculty to permit the division for the first three classes by subjects instead of by numbers, thus starting a much needed change. After one year of service he was so troubled by increasing deafness that he resigned from Yale and threw himself heartily into the instruction of the deaf mutes at the Hartford Institution. Removing in 1832 to the similar institution in New York city, he labored zealously and happily until his call to the University of Alabama early in 1838.

During the sixteen years of his stay at Tuskaloosa, Barnard began the campaign for good

discipline and a correct curriculum, which only ceased when he resigned the Presidency of Columbia in 1888. He was specially occupied with the teaching of chemistry, natural philosophy, or mathematics, but made time for some outside scientific work, as, for example, the commission to establish the boundary between Alabama and Florida. His chief energies, however, were devoted to the old problems of discipline and curriculum and many letters, editorials and reports attest his activity as well as his great power in this field of discussion. Incidentally he frequently took occasion to inveigh against secession, and in vain endeavored to cultivate devotion to the Union, especially in his famous Tuskaloosa oration July 4, 1851.

In 1846 he was married to Margaret McMurray, a young woman of English parentage, who was ever to him a loving and devoted helpmate, and to her affectionate zeal is chiefly due this collection of memories. After his death, April 27, 1889, she made arrangements to publish his life, but when only two chapters had been written she suddenly died, leaving the editor uninstructed in details and unprovided with many important letters.

In 1854 Barnard was called to the chair of mathematics and natural philosophy in the University of Mississippi, at Oxford. During his first year he also gave full courses in chemistry, astronomy and civil engineering. He was soon after elected Chancellor (President) of the University, in which office he labored still more zealously and effectively for good discipline and for the true university. This period is especially noteworthy as witnessing his change of views with reference to the relative importance of 'mental discipline' studies like the classics and of the 'useful' studies like science.

While chancellor he was on one occasion charged with 'unsoundness' on the slave question, a charge of which he was acquitted by unanimous vote of the trustees. Even though we must give full weight to the trying conditions amidst which he was placed, still we must admit that in this instance he lacked that supreme element of courage which would have boldly proclaimed the abhorrence of that institution which his earlier and later utterances show that he must have felt. Instead of this he allowed appearances to exculpate him, without any open declaration that would contravene his secret convictions. This weakness was emphasized by the publication, after his return to Washington, of a most rabid attack upon slavery in his 'Letter of a Refugee.'

At the final outbreak of the war he resigned the Chancellorship and left Oxford in 1862, eventually reaching Washington, where he was occupied with several pieces of scientific work until his appointment, in 1864, as President of Columbia College, in the City of New York.

The twenty-four years of his Presidency of Columbia were years of hard work, with many discouragements, but much success, and closing with the college in a position from which it could and did suddenly rise to the rank of a University of the first class. During this time he labored for the true university and argued as forcibly for an optional course, and for the advantages of the exact sciences, as he had previously insisted upon an inflexible devotion to the classics at Tuskaloosa. He seems actually to have changed his opinions upon this subject, but was not willing to admit it, striving to ascribe the needed change of course entirely to changed conditions. Indeed his love of science is well proved by his generous bequest to Columbia of a library fund of $50,000, from the income of which the Barnard medal ($200) is given every five years, and a $10,000 science fellowship fund.

Latterly, he urged the admission of women to the privileges of the college and university, and Barnard College is at once the result and the reward of his activity in this field.

Although a little prolix in places, and the introduction of quotations sometimes results in repetition, still these memoirs may be read with pleasure and profit by all who are interested in the progress of this country during the last three-quarters of a century. A brief but interesting history of Columbia College is introduced, taken largely from Dean Van Amringe's more elaborate sketch.

Barnard appears to have been a man of considerable power, rather dogmatic and somewhat dictatorial, but usually supported by good

reasons. In fact it seems doubtful whether his strength lay so much in the inherent correctness of his ideas, as in the uniform clearness and force with which he propounded and defended them. He was a born advocate, and if he had been able to follow his chosen profession of law he would undoubtedly have become famous. W. HALLOCK.

COLUMBIA UNIVERSITY.

The Legend of Perseus; A Study of Tradition in Story, Custom and Belief. By EDWARD SIDNEY HARTLAND, F.S.A. Vol. I. The Supernatural Birth. London, David Nutt.

Mr. Hartland believes that the classical myth of Perseus belongs to a group of folk tales ranking among the foremost in interest for the student of the evolution of human thought and human institutions. The first three chapters are devoted to an account of the story as given by the poets and historians of antiquity, and in modern folk-lore; the remaining chapters trace the supernatural birth in Märchen, Sagas and practical superstitions. The legend consists of three leading trains of incident, viz.:

1. The Birth, including the prophecy, etc.
2. The Quest of the Gorgon's Head, including the jealousy of Polydectes, etc.
3. The Rescue of Andromeda, including the fight with the monster, etc.

It is considered that the modern tales have come down from classical antiquity in the countries in which they are now found, but they are subject to variations. After the Danæ type we have a type in which the wife of a poor fisherman eats the head of a fish—the king of the fishes—and becomes the mother of three boys. In this group the plot consists of four incidents, distinguishable as:

1. The supernatural Birth,
2. The Life-token,
3. The Dragon-slaying, and
4. The Medusa-witch.

After this group there still remain a large number of variants, wherein one or more of the incidents are wanting or may be represented by a mere relic. Some of these the author recognizes as probably derived by degradation from one or other of the earlier versions; of some he is constrained to say that they are in a state of decay; and in some the reader can hardly see any resemblance at all to the legend of Perseus. Yet the connection may be said to be made out, through the tracing of the gradations of change.

The stories of supernatural birth are very numerous, and may be said to have a currency as wide as the world. The usual agency is, that a woman eats some part of a mysterious fish; but in India it is fruit that she eats, an apple or an orange, or two grains of wheat, or soma seeds; or she swallows a potent drug. Conception in other cases has been by the wind or by the rays of the sun. The author has been very industrious in collecting stories and very discriminating in their classification; he gives more than twenty pages of authorities whom he has consulted; and as a student of folklore, working on approved lines, he has performed his task well.

The question, however, occurs, whether this laborious hunting up of stories is a very profitable business? Many of the modern folk-tales may be interesting stories for the nursery; and it is as well, once for all, to know their relations or resemblance to the legend of Perseus; but what was the meaning of the Perseus legend itself? We are not persuaded of the 'anthropological' explanation, according to which 'the original belief is intimately bound up with the savage theory of the universe.' The Greeks had ceased to be savages when they came to believe in Zeus, and framed the story of Perseus. The Greek mythology had an astronomical basis, and not an anthropological; Perseus and Andromeda are still constellations in the heavens; and Zeus is there too, though unrecognized. Besides, it hardly seems consistent to trace the folk tales of savages to the Greek myths and then seek the origin of the myths in the irrational fancies of savages. The study of folk tales, in their multiplication, variation and decay, is analogous to the study of Scripture MSS. with their hundreds of various readings. The revisers of the Bible found that the three oldest manuscripts were of more value than all the hundreds of later copies; and the student of the legend of Perseus will find the parallel Babylonian legend of Gilgames more to his purpose than a bushel of modern folk-lore.

GEO. ST. CLAIR.

SCIENTIFIC JOURNALS.

TERRESTRIAL MAGNETISM, JULY.

A Summary of the Results of the Recent Magnetic Survey of Great Britain and Ireland Conducted by Professors Rücker and Thorpe: By A. W. Rücker.

The writer divides his article into three parts.

Part I: *On the Accuracy of the Delineation of the Terrestrial Isomagnetic Lines.* Complete observations were made at 882 places in the British Isles, thus averaging one station to every 139 square miles of land area. With the exception of the recent magnetic survey of Holland by Dr. Rijckevorsel, where the stations averaged one to about every 39 square miles, this survey of Rücker's and Thorpe's is the most detailed one thus far made. A first survey embracing 205 stations was made for the mean epoch 1886, and a second one covering 667 stations for the epoch 1891. In no previous case have *two* such detailed surveys for the same region been made within so short an interval of time. They they therefore present a good opportunity for testing the accuracy with which the positions of the terrestrial isomagnetics can be inferred from the observations. The conclusion is reached that the accuracy of the calculated values is about equal to the probable error of an observation (declination $\pm 0.'6$, dip $\pm 0.'4$, horizontal intensity in C. G. S. units ± 0.00006).

Part II: *On the Accuracy of the Determination of the Local Disturbing Magnetic Forces.* The main object of so elaborate a survey was to study local magnetic disturbances. For this purpose the northerly, the westerly and the vertical components of the earth's magnetic force were determined from the observed elements, declination, dip and horizontal intensity. The differences between these quantities and those calculated from the formulæ for the isomagnetics gave the corresponding components of the disturbing force. When these were plotted, and lines called respectively *ridge* and *valley* lines were drawn through the loci of maximum and minimum vertical disturbing force, it was found that, with few exceptions, the horizontal disturbing forces were directed to the ridge lines. Eight such regions were detected in the 1886 survey and again revealed in the later survey. The range of the magnitude of the vertical disturbing force at places where the surface is comprised of sedimentory rocks is about 0.00600 C. G. S. units. On granite and gneiss the range is doubled, and in the neighborhood of basalt it may be enormously increased. One of the ridge lines could be traced without difficulty for 170 miles and more. Disturbances of a secondary order were also revealed.

Part III: *On the Relation between the Magnetic and the Geological Constitution of Great Britain and Ireland.* The most probable causes of the disturbing forces are electrical earth currents and magnetic rocks, or both of these combined. Rücker is led to the belief that the presence of magnetic rocks is the more potent cause. The article is illustrated by maps and concludes with a summary of the results with regard to the relationship between geological and magnetic features.

Die magnetischen Störungen der Yahre 1890–5, nach den Aufzeichnungen des Magnetographen in Potsdam: By G. Lüdeling.

This is an investigation of the magnetic perturbations for the interval 1890–95, as revealed by the self-registering instruments at the Royal Magnetic Observatory, Potsdam. According to the tabular and cartographic presentation, there is a characteristic increase in the number of disturbances towards evening and decided decrease at noon. The equinoctial months are the most disturbed, and in these the diurnal distribution of disturbances is most pronounced, a double maximum showing itself most decidedly. The annual distribution as determined from this six-year series shows as yet no pronounced parallelism with the distribution of sun spots over the same period. There is, however, a nearly perfect concordance in both the diurnal and in the annual period of magnetic disturbance and of polar lights.

A letter to the editor by Prof. Hellmann of Berlin, with regard to an old work containing magnetic declinations, notes by the editor and reviews of Paulsen's papers 'On the Nature and the Origin of the Aurora Borealis,' of Carlheim-Gyllensköld's 'Determination of the Magnetic Elements in Sweden and of Weyer's Researches,' 'On the Magnetic Declination and its Secular Variation,' conclude the number.